FARADAY'S DIARY

VOL. VII

MICHAEL FARADAY

FARADAY'S DIARY

Being the Various Philosophical Notes of Experimental Investigation

made by

MICHAEL FARADAY
D.C.L., F.R.S.

during the years 1820–1862
and bequeathed by him to the
ROYAL INSTITUTION OF GREAT BRITAIN
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THOMAS ~~MARTIN~~, M.Sc.

with a Foreword by
SIR WILLIAM H. BRAGG, O.M., K.B.E., F.R.S.
Director of the Laboratory of the
Royal Institution

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PREFACE TO VOLUME VII

IT was mentioned, in the preface to Volume I, that folio volumes I to VI of the manuscript Diary were bound by Faraday, and that folio volumes VII and VIII were made up afterwards from the loose scientific papers left at his death. Of this accumulation of papers, everything seems to have been treasured, and bound into the last two volumes of the Diary; with the result that folio volume VII contains a small amount, and folio volume VIII a great deal, of miscellaneous material having little or no relation to the contents of the earlier volumes.

It is very unlikely that Faraday regarded all these miscellaneous papers as being included in the "philosophical notes of experimental investigation" he bequeathed to the Royal Institution; and a considerable part of them is obviously unsuitable for publication, as for example, a quantity of index slips which have been pasted on to foolscap sheets. It would seem that Faraday made some progress with an index to the Diary, but did not complete it. The slips are not in order, and the words: "Slips left loose and unarranged" have been written on some of the sheets, apparently by Mrs Faraday. Notes and jottings of all kinds on scraps of paper have been preserved in much the same way, and there are some longer manuscripts; but comparatively little of this material relates to actual experimental work.

The long sequence of numbered paragraphs of the Diary continues in folio volume VII up to 16041, on March 6th, 1860, when it ends. The last sixteen or so pages of this volume consist of the index slips, and have not been printed. Of the contents of folio volume VIII, a selection has been made, on the principle that the material to be printed must be of the same general character as that in the earlier parts of the Diary, that is to say,

notes in Faraday's own hand of experimental researches upon which he was personally engaged. The items thus chosen, and printed in this volume, are as follows:

(i) Experiments on telegraph wires, made in 1853–4 with the co-operation of Mr Latimer Clark (pp. 393–411).

(ii) Experiments on the electric discharge in vacuum tubes, made in 1858 in association with Mr J. P. Gassiot, F.R.S. This work was principally Gassiot's, but the extent of Faraday's collaboration was considerable. Gassiot published two papers[1] describing the experiments, but Faraday kept his own notes of them, in a separate numbered series (pars. 1–292, pp. 412–461).

(iii) Experiments in 1861–2 with Steinheil's apparatus. That on March 12th, 1862, is probably the last experiment Faraday ever made (pp. 462–5).

Of the material in folio volume VIII which has not been printed, a considerable part, occupying about two-thirds of the whole volume, is evidently the contents of a loose file of odd papers and notes. Prefixed to it is a table of contents, in Faraday's hand-writing, headed "Subjects to work and think out". The most considerable item in this section is a long "Description of a magnetic torsion balance", apparently the rough draft of a paper which was never published. The instrument described is that referred to in the Diary entry of July, 1852 (Vol. VI, p. 73, par. 12020) and used in the magnetic measurements during the autumn of that year. The other papers not printed include:

(i) Note on regelation. This is the rough draft of Faraday's last communication to the Royal Society.[2] The experimental work referred to is described in the Diary (pp. 382–390).

(ii) Note on the possible relation of gravity with electricity or heat. This is the manuscript of a short paper which, according

[1] *Phil. Trans. R.S.*, 1858, pp. 1–16 and 1859, pp. 137–160.
[2] *Proc. Roy. Soc.* x, 1859–60, pp. 440–450.

to Bence Jones, who gives an extract from it in his *Life and Letters of Faraday*, was sent to the Royal Society in June, 1860, and subsequently withdrawn on the advice of Professor (afterwards Sir George) Stokes, as containing only negative results. The experiments upon which it is based are those made in 1859, and recorded in the Diary (pp. 334–381). It has been thought best that the paper itself should remain unpublished.

The index to the complete Diary, volumes I to VII, will be found in a separate Index Volume.

I wish now to thank all those who have been associated with me during the past five years in the work of producing these volumes. Mr A. J. V. Gale has assisted me from the beginning. His interest and enthusiasm have lightened the labour we have shared, and I am greatly indebted to him. Mr Gale has that faculty for detecting the errors in a page of print which is at once the envy and the despair of less skilful readers, and has contributed largely to whatever qualities of accuracy and integrity the text may possess. To Mr R. Cory, who has supervised the preparation of the photostat prints from which the blocks have been made, I am most grateful. Mr W. J. Green's knowledge of Faraday's experiments, which he has allowed me to draw on from time to time, has often helped in the elucidation of doubtful passages; and other members of the Staffs of the Royal Institution and the Davy Faraday Research Laboratory have given their assistance in various ways.

The interest and advice of Mr G. H. Bickers on every point in connection with the publication have been invaluable, and my thanks are specially due to him and to the staff at Messrs G. Bell and Sons, who have borne with me with exemplary patience. Of the printing of Mr Walter Lewis and the Cambridge University Press the book, I venture to think, speaks for itself; and the demands

on the printer have been exacting, in a work in which many of the ordinary rules of composing have had to go by the board. Messrs Emery Walker Ltd. have made the excellent line blocks and collotype plates.

To Sir William Bragg and the Managers of the Royal Institution I am deeply indebted for the opportunity of carrying out a task which, if it has seemed at times a feat of endurance rather than a literary work, has been a most valued privilege.

T. M.

ROYAL INSTITUTION
September 1935

CONTENTS

Cross references to published papers are, as before, to the collected editions of the *Experimental Researches in Electricity* and the *Experimental Researches in Chemistry and Physics;* except papers published after these volumes had appeared. References are given for these to journals in which they may be consulted.

To avoid repetition, only a single reference has been given below to Faraday's paper on the Experimental Relations of Gold (and other Metals) to Light; but it is to be understood that the paper is based on the whole of the work carried out from February to December 1856, and recorded in pars. 14243 to 15403.

PLATES

FOLIO VOLUME VII OF MANUSCRIPT

(CONTINUED)

24 NOVEMBER 1855.

14201. Static induction experiments in relation to Riess' letter—Phil. Mag., 1855, IX, p. 410, etc.

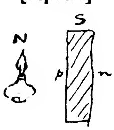
14202*. Let P represent the inductric body—S, the insulating plate whose state under induction is to be investigated an[d] N the flame or other inducteous body, which is to be uninsulated and to act as a discharger—as in Riess' experiments.

14203†. Made P the end of the E. Machine prime inductor, N a spirit lamp according to Riess, and S a plate of shell lac $4\frac{1}{2}$ inches square and 0·9 of an inch thick. A thick silk thread was fa[s]tened round it and two silk thread loops proceeded from the four corners, by which it could be held in any position and yet remain insulated. The side of this plate facing towards the inductric body shall always be called the anterior side—the other, the posterior side.

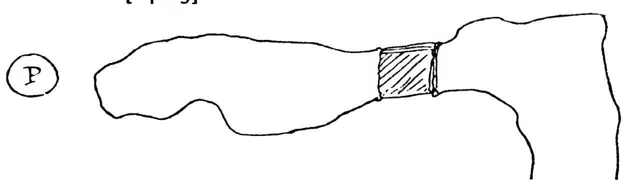
14204. The shell lac S was first discharged by the near approach of the flame N to both sides of it, P being away, and then examined by being laid on to the plate of a gold leaf electrometer—it was found to possess *no* charge and could at any time be brought into this state by a like process. Then the flame N was removed and the plate S brought into inductive position as regarded P, left there 5 or 10 seconds, taken away and again examined, and was found without charge. It was replaced in inductive position and the hand applied at N, so as to perfect the induction arrangement but so that the hand should not communicate electricity; after which the plate S was removed and examined by the electrometer, but had received no charge, though it must have suffered induction.

14205. Then the shell lac plate S was put into position in respect of P, and afterward the flame N brought up to within an inch and a half of it, and moved about for a second and removed; and then the shell lac plate, being taken away from P, was examined at the Electrometer. Whichever side was laid on the electrometer cap plate, still the shell lac was found to be charged negative, P being Positive, but the charge was much stronger in its indications when the posterior surface was against the Electrometer cap, as according to my theory it ought to be.

* [14202]



† [14203]



14206. The next point was to ascertain whether the anterior surface was charged Neg. as Riess says—or the posterior surface negative as I believed, and the result was soon obtained. For on bringing up the flame towards the posterior surface (14202, etc.) to discharge it (if charged) and then examining the shell lac at the Electrometer, it was found uncharged, and that whichever side of the plate was laid on the cap. If the anterior surface had been really charged Negative as Riess supposes, the approach of the flame to the posterior surface would have given the latter a positive charge, and these two charges would have acted on each other inductively across the plate; and then on putting the plate on the electrometer cap, the charge which might appear strong before the second action of the flame would have appeared weak afterwards; but then the approach of the hand or a metal plate uninsltd., by taking of[f] the induction of the upper charge, would allow the lower to be exerted on the electrometer and shew its full power: also by this process opposite kinds of electricity would appear on opposite sides of the shell lac plate. But the approach of the hand did not produce any effect of this kind, whichever side of the plate was downward, for the plate was wholly discharged. Hence therefore it was the posterior side of the shell lac that had been charged Negative by the flame under the action of induction, and it was the same surface which was discharged by the second approach of the flame when the induction was removed.

14207. To complete this proof, the experiment was repeated up to the second discharging action of the flame—and at that time the flame was brought up on the *anterior* side of the shell lac plate. The latter was then examined at the electrometer with its posterior surface on the cap; the charge was Negative and therefore the same in kind as before, but the effect much lower. On approaching the hand or an uninsulated conducting plate towards the top or anterior surface, the negative indication rose up almost as high as in the first instance, and on removing the hand, etc. it fell again. On turning the shell lac plate round so that the anterior surface was on the cap, all the effects were repeated, the charge indicated by the electrometer being now of positive electricity.

14208. Hence it is very clear that the first action of the flame (under induction) was to charge the posterior face of the shell lac negative:—that when the induction was removed, that posterior face remained charged and could then act by induction on things around, either through the air on one side the face or through the shell lac and air on the other side. That when the flame was approached on the air side, the induction across the air was discharged by the flame, and so the charged face itself discharged; but that when the flame was approached on the other side, i.e. the anterior side, the induction was through both air and shell lac; that the flame could discharge that across the air, but because of the immovability of the particles of the lac, not that across its surface; and so the discharge ended on the surface of the shell lac, and it acquired the positive state, i.e. the state the hand or any undischging. body would have received put in the place of the flame, and the state the flame itself has before it can exert any power of discharge.

14209. Instead of employing a flame for N, I used a fine needle point, with precisely the same results, except that the needle point is not so good a discharger by convection as flame is; the different states assumed by the shell lac plate are not so readily assumed or discharged by it as by the flame.

14210. It is evident that if in the second step of the experiment the spirit lamp as a discharge[r] of the posterior surface were dispensed with, and a conductor applied to that surface, it might be made to take all the electricity at once. This was done by laying a gold leaf on the cap of the electrometer and putting the plate of Shell lac from position S with the posterior face down upon it. The electrometer was then well diverged negatively, and by bringing an uninsulated wire up to the gold leaf or the cap in contact with it, that negative electricity was drawn off as a spark and the plate and electrometer was left entirely discharged.

14211. It is just as evident that the metallic coating may be applied to the posterior surface before the experiment begins. So a plate of tin foil was attached to this surface and then the experiment made, using the spirit lamp at N first to charge that surface under induction, then to discharge it when the induction was over; the effects were precisely of the same kind as before. But then

varying the experiment by using an insulated metallic knob for N, the negative electricity going from N to the posterior surface whilst under induction, and that going back again when the induction was taken off, occurred as two visible sparks.

14212. It was but another variation of the experiment with precisely the same results in kind, when both surfaces of the shell lac were coated; that only made the extreme limits of the shell lac conductors, whilst all the intervening parts acted as insulating matter in the induction polarized condition. The consequences were the same. The plate being put into position and the flame applied at N and then removed and the plate then examined, it was found charged negative on the posterior side; a touch of the wire there took away this negative electricity and the whole system was discharged, no electricity remaining in the anterior surface. But when the flame was applied on the anterior side, that lining became positively charged, whilst the posterior lining remained negatively charged, the shell lac being in the inductive and polarized state between.

14213. When P was made a negatively electrified body, then precisely the same effects occurred, but the electric states conferred or removed had the contrary signs. Still, it was always the posterior surface which became first charged under the induction.

14214. Then S was changed from Shell lac to Sulphur, the plate being 5 inches square by 0·8 of an inch thick. All the experiments were repeated with precisely the same results; there was not the slightest difference in the kind of action.

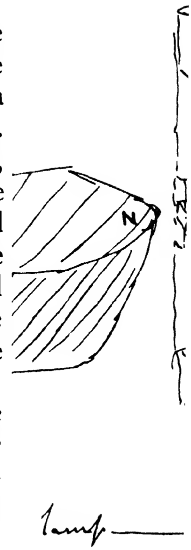
14215. P may be any electrified body. A gutta percha sole excited may be used and was used in all these experiments. It may be brought within an inch of the anterior face of the shell lac or sulphur—it may even touch it (without friction)—and not charge it in the least. Still, the point or flame or knob on the posterior side, i.e. the inducteous body, will do its full duty in charging or discharging the posterior side, so that there is no difficulty in making all the experiments with ordinary care.

14216. P may be an excited rod of glass or any like charged body and is easily brought up at the moment the induction is required. So that the experiments become very easy and simple,

and they are fundamental as regards the true action in and theory of induction.

17 DECR. 1855.

14217. Polarized ray of light through Magnecrystals in the magnetic field. The object was to place a magnecrystal in the strong magnetic field in different positions and then pass a polarized ray through it at right angles to the magnetic axis. For this purpose the Great horseshoe electromagnet was used, excited when needful by 20 pr. of Grove's plates. The Pole pieces N and S were employed (   ). A brass tube sustained between them carried two Nicholl's prisms, one below to polarize a reflected ray of lamp light, the one above to serve as the optical analyzer. An opening in the middle of this tube and a little stage there sufficed to permit the introduction and arrangement of the crystal to be examined between N and S.



14218. Various crystals, of *Sulphate of iron*, *red ferro prussiate* of potassa, etc. had the required parts rubbed down on sand paper and then by means of a piece of linen stretched over a flat plate and damped in one part, the surfaces could be finished and polished so that an excellent image of the lamp flame could be seen through the crystals as through coloured glass.

14219. *Sulphate of iron crystals* were first dealt with–some were in flat plates, with the polished surfaces parallel to the Magnecrystallic axis of the crystal. Others were formed into square prisms, of which two of the faces were parallel to the Magnecrystallic axis and other two faces perpendicular to this axis.

14220. First the Analyzing Nichol was turned so as to be at right angles with the Polarizing Nichol, i.e. until all light was extinguished; the tube with the two Nichols could then be turned altogether and of course no ray passed during the whole time. Then a crystal could be placed on the stage between the Magnetic poles, and this depolarizing the ray from the lower Nichol, suffered light to pass through the upper. By turning the experimental crystal round on a vertical axis, it could be brought into one of two diametral positions in which it did not affect the polarized light and then all remained dark, and by turning the whole system this arrangement could be preserved and yet the

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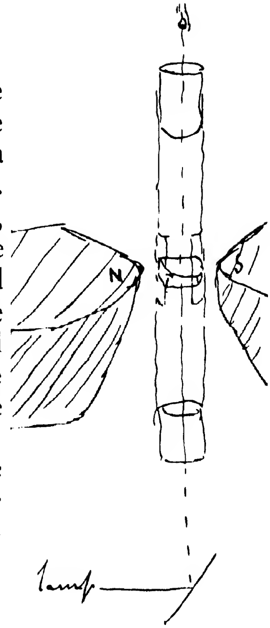

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Magnecrystal be placed with its M. C. axis parallel to the Magnetic axis, or transverse to it, or in any other Azimuthal position.
14221. Crystal A, Sul. Iron. A flat crystal with the M.C. axis parallel to the faces and placed horizontally—adjusted so as not to affect the polarized ray—all dark. When all was turned until the M.C. axis coincided with the Mag. axis of the field and then the Magnet made and unmade, there was no difference—no magnetic effect. When all turned until the M.C. axis was equatorial and then magnetic alternations made, still no effect. The tube and crystal was turned through every azimuthal position and the magnet force intermitted—but still no effect. The upper Nicholl was turned a little round—or much round—but still the magnet produced no effect.
14222. Crystal B of proto sul. Iron—a square prism polished on four sides ( ). Adjusted with Nicholls arranged to stop light—the crystal so as not to alter the polarized ray, and its M. C. axis horizontal—then the whole turned through all Azimuthal positions and the magnet continually made and unmade—but no effect. The crystal now arranged with the M.C. axis vertical. Same negative effect as before.
14223. Crystal C, proto sulphate of iron—flat plate—M.C. axis horizontal. Negative results exactly as before.
14224. Crystal D, proto sulphate of iron—plate—M.C. axis horizontal. Negative results as before.
14225. Crystal E, proto sulphate of iron—square prism—but whether the M.C. axis were vertical or horizontal (14219), the results were negative as before.
14226. Crystal F. Small long prism, Sul. iron.—M.C. axis horizontal—the same negative results as before.
14227. *Red ferro prussiate of potassa.*

Crystal G, red ferro pruss.—also crystals H, I and K—four being employed in succession. The M.C. axis was horizontal—but in no azimut[h]al position had they power under the magnetic influence to affect the ray of light—all the results were negative.
14228. *Calcareous spar*—a cube—the optic axis being perpendicular to two of the faces. When the optic axis was horizontal, then dark positions could be obtained—but no effect was produced by the magnet. When the optic axis was vertical—light and coloured

stripes or rings were obtained—but these underwent no change by the action of the magnet. All variations of the positions were made, but no magnetic effects produced.

14229. *Ferro calcareous spar* (14185, 95). Two little rhomboids were selected and cut so that the optic axis in one was parallel to the new faces of the plate produced, and in the other, vertical to these faces. But which ever crystal was in the Magnetic field, or whatever its position, no magnetic effects could be obtained.

14230. So whatever the position[s] of these Magnecrystals, paramagnetic and diamagnetic, were in the field—no magnetic effect could be obtained on a polarized ray passed through them in a direction perpendicular to the magnetic line of force.

19 JANUARY 1856.

Time—in Electrodynamic or Magnetic induction.

14231. The facts Exp. Res. 2166, and also MS. notes 7500–7701, as well also as the former (   ) negative results with a rapidly revolving cylinder of heavy glass employed as a prism in the magnetic field, have a relation to time. As the effects looked for were insensible, so the results, as far as they go, are against any expectation of sensible time results in other trials.

14232. Professor Henry (Princetown) says I think that lightning flashes ten miles off affected his induction coil apparatus. I do not know how; but if the results were instantaneous they are against the idea of sensible time.

14233. Still, it is not absolutely certain that magnetic propagation, if in an (or the) ether, must be as quick as light, though it is likely; and the experiment is worth trying; especially as the magnetic influence is *transverse* to the electric current, which rivals light; and time is certainly required in some magnetic phenomena, as in the charging of soft iron.

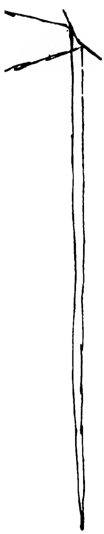
14234. Considering also that the magnetic form of force moves matter and masses of matter: that point is perhaps in favour of *time*, for all actions that result in moving ponderable matter, as far as we can trace them by cessation and renewall, seem to occupy sensible time.

14235. The thought at Exp. Res. 1730, as to the difference of copper and air when employed as screens, may have a reference

to this point of time—and perhaps such interposition and its effect on time might be made sensible by the apparatus I propose employing.

14236. In reference to the indicating galvanometers before thought of (13993). The image reflected by the moving needle may require to be very intense. So make the object seen *a Voltaic light*; it will be both small and intense.

14237. Let the needles be very hard, so as to hold a full charge of power. Also as short and as light as possible that the inertia of their mass may be the minimum—and I think astatic or very nearly so.



14238. Both have to be observed at once, i.e. both Galvanometers. As the size of the coils and apparatus will prevent them from being placed so near to each other that the two reflected images shall be seen close together, so it may be necessary to have two reflectors to turn the rays at right angles, or even between the instruments, and so adjusted as to give the two images side by side and close together to the eye.

14239. Small governing magnets (very small) might be used with each galvanometer in adjusting the place of the place [? needle] and of the image reflected from it.

14240. To distinguish whether one needle moves before the other, the images from the two placed side by side must be observed by a moving mirror or lens. An excentric mirror or prism would give a certain separation of *successive* extinctions, but probably a plane mirror will be best. For it will be easy to move a mirror rapidly on an axis corresponding to one of its diameters, and to make the mirror carry a contact key so that the primary or inducing current and the motions of the needles shall come on exactly when the mirror is passing that position in which the light is reflected to the eye.

14241. We might perhaps in this way distinguish the ten-thousandth part of a second. But there is little likelyhood of that being enough, since light would move 19 miles in that time, and electricity in a copper wire as fast.

14242. Still, the apparatus may give some results, and especially when interposed media as copper, air, bismuth, phosphorus, etc., conductors and nonconductors, etc. are concerned.

14243. Wishing to look at Gold leaf in a good Microscope, I applied to Mr W. de la Rue, and he has undertaken to aid me with his instrument. Went last Tuesday Evening (28 Jany.) to his house for that purpose and we had a good evening together.

14244. Looked at ordinary gold leaf (beaten) spread both on glass plates and also over small holes in cards. A light behind such gold leaf appears to be green, as if the Gold were a green transparent body; it usually appears to be wrinkled and to have many holes, the latter transmitting white light. On using a lens the same results appear, but the wrinkles are more developed and the holes larger.

14245. Being placed in the microscope and examined by transmitted light (sometimes direct from a lamp and sometimes reflected by a mirror) the development was made more complete, higher powers being employed in succession. At first fresh wrinkles and holes appeared—the holes shortly ceased to increase in number but the gold seemed irregular in form, an effect due to wrinkles, folds and undulation in a large degree, for by the highest powers these were opened out and then parts could be distinguished pretty equable and flat over a certain extent and having every character of a continuous transparent film of the metal—no holes appeared there but there were mottlings as if the plate of gold were irregular in thickness.

14246. Such irregularity is very like to be, in gold beaten out by such a rough process as hammering and between leaves so coarse in their nature as gold skin, but the Green tint of the light was on the whole nearly the same though in the thinner parts it seemed to change to a purplish or greyish hue. On the whole it is wonderful to see the leaf so uniform as it is in thickness.

14247. On consideration, it does not seem wonderful that the gold should retain its continuity and be almost free from holes. The mere expansion by beating would not tend to make holes but rather weld together and close up any that might be formed in the transferring processes. Beating would, by the direction of the forces laterally, favour the continuity of the film—not destroy it.

14248. The last power used was such as to make the microscope able to shew perfectly lines and spaces $\frac{1}{70,000}$ of an inch asunder. Lines ruled by Nobert and others thus close have been distinguished and the spaces between them; and if it be assumed that the lines and spaces were of equal width, then the microscope could shew spaces $\frac{1}{140,000}$ of an inch across. Yet as before said, holes had not gone on appearing in greater numbers and of smaller sizes. The smallest could be seen well by a much lower power than that described.

14249. Every consideration and appearance leads to the conclusion that the Gold is truly transparent.

14250. The same general results were obtained with red gold—with high gold and with pale gold. Each kept its own tint throughout the observations, but the close observation, with the highest power, of the colour of the light in the different parts of the mottle was not made.

14251. A film of gold leaf was wetted with Alcohol and then examined: nothing particular appeared—a little change in the tint of light perhaps occurred, as was probable.

14252. A polarized ray of light was sent through the gold leaf and then examd. by an analyzer—both the polarizer and analyzer being Nicol's prisms. Nothing distinct appeared. At times there were suspicions of a little depolarizing action, but not satisfactory.

14253. By the adjustment of a sulphate of lime plate, the colour of the light sent through the plate could be varied, and then the portion transmitted by the gold leaf varied: it could be changed from green to gray and in other manners, and it was evident that other rays than the green rays could pass through the gold. The yellow was probably the ray first stopped back—and that is also accordent with the complementary characters of the reflected and transmitted light of gold leaf.

14254. It is especially necessary in these experiments to exclude light from the surface of the gold leaf nearest to the eye, for the corrugations and undulations of this surface easily catch such light and reflect the rays to the eye, and then a mixed and confused result is obtained.

14255. Propose to reduce films chemically on glass or otherwise—and also of other metals.

14256. Remember the transparent films obtained with a lead tree (   ).

14257. Yesterday (Feb. 1) Mr De la Rue brought me a series of thin films of gold attached to glass plates—obtained by the reducing action of phosphorus vapour on solution of gold—which had been washed by water and then placed on the glass. They are beautifully thin. Yet when examined by a microscope of high power, they appear to be perfectly continuous and uniform.

14258. Another consideration in favour of their continuity is that the process of formation is uniform on every part of the reduced or reducing solution. Also that they have considerable strength on water.

14259. I have frequently observed that when a piece of gold leaf lying on the surface of a solution of chlorine or upon mixed Nitro muriatic acid is undergoing solution, the plate in gradually thinning becomes more and more transparent—changes in colour—assumes the purple colour of these plates and even passes into a thinner and almost colourless plate before it disappears. This a very good thinning process in proof of the philosophy of the subject.

14260. Precipitate of Gold by Sul. Iron gives green and blue and purple tints. These probably all cases of thin gold and the transmission of light through the particles.

14261. As these gold films on glass (   ) are doubtless conductors of Electricity, we shall have the power of throwing more gold down upon them by Electricity, and so of procuring thicker films—or if we please, wedges, so as to have transparency in a plate gradually increasing in thickness. The films of Gold thrown down on Daguerretypes are transparent films.

14262. Might also thin down a gold film by electricity. Might thin down a piece of Gold leaf, i.e. beaten gold leaf.

14263. The thinner of these films transmits the light of the clouds or of a piece of white paper to the eye of a fine violet colour, so that the film has exactly the appearance of a piece of manganese glass. It is the same colour as the stains of Gold on the skin or other organic substance—or the stains obtained by gold on pottery—or the stain of an electric deflagration of gold—

and I have little doubt that in all these cases the cause is the same—the effect being due to the colour of thin transparent plates or particles of gold.

14264. When the light of the Sun or day is sent through these films on to white paper, it illuminates the paper with a fine violet light. Candle light has but few of these rays and therefore produces little, i.e. a worse effect of the kind.

14265. One film varied in thickness from one end to the other. At the one end it was so thin as to be transparent almost as the glass, and here the transmitted light (once transmitted) had a rose tint; this gradually passed into the violet tint towards the other end and even reached the green tint or violet green tint at one part. The effect due to increasing thickness of the plate was very beautiful.

14266. When a film and glass was laid on white paper so that the light which went to the paper and from thence to the eye passed twice through the gold, then the colour was more intense. So if a film were taken up on white card board instead of glass, it gave a strong violet tint, and in the place of the thicker part the tint became so dark as to apprch. to deep violet brown or black. It did not in these places exhibit the green tint of the thicker films of gold.

14267. When the spectrum of a candle obtained by a prism was looked at thrgh. the gold film, all the rays passed thrgh. the thinner part, almost as thrgh. the glass. As the thicker part was brought up, the yellow rays appeared to be excluded first.

14268. In considering the proportion of rays which pass through a film of given thickness, we must remember their proportion in the spectrum and not merely their colour. Violet appears to pass as freely as, if not more freely than, any others; but then its quantity in the ray is small estimated by illuminating power and so it disappears before the green, which has a high illuminating power, and thus green appears as the tint of the thicker films or of gold leaf.

14269. Even Stokes' rays can pass through these films and affect sol. of Sulphate of quinia afterwds.

14270. As to *reflexion of light* by these films, it is very good where they are thickest, and indeed perfectly golden in parts where still

transparency is manifest, though the dark slate or greenish colour by transmission is very dark. Proceeding to thinner parts of the film, the reflected light becomes less as the transmitted light is more, and at the thinnest part almost disappears, becoming to gold what the top of a soap bubble is to water—*too thin to reflect light.*

14271. But if that be so and if these films are continuous and not porous nets or aggregations of particles, then it is clear that reflexion is not altogether superficial, but is an internal action of the gold: mere surface is not enough, for a thin film, though it gives a metallic and yellow reflexion, gives it in so poor a degree that it almost disappears, and it is only as the film becomes thicker that the reflexion becomes the ordinary strong metallic reflexion of gold.

14272. So thickness governs reflexion in a large degree.

14273. The undulations for violet light are 59880 in an inch and for red light 37600. The thickness of Gold leaf is given as $\frac{1}{282,000}$dth of an inch in thickness, so that it is not $\frac{1}{7}$th the extent of a red vibration and little more than a fifth of a violet vibration. Some of the films are probably not the $\frac{1}{20}$ part of the gold leaf in thickness.

14274. If thickness of continuous film has an influence on reflexion, then it is probable that a given film will affect differently rays of different undulations, such for instance as are above and such as are below that thickness.

14275. Superposition of two or more thin plates of gold does not seem to change the colour to that of a thicker plate: thus several purples do not give a green colour. This would seem to imply that it is not sum of thickness only that transmits a green colour but sum of *continuous thickness.*

14276. As reflexion also required a certain depth of gold, so two thin plates there cannot be as one thick one. Hence with very thin plates, light may pass on which would be reflected or absorbed by one thicker one—and hence probably the reason for the constant colour of several thin plates—of gold precipitates, etc.

14277. *Continuity of the film* an important point to prove. Perhaps take up one of the films on polished silver, then cause sulphur vapours to be present and see if the gold film will protect the silver.

14278. If a wedge film obtained by chemical action or by electrical action proceeds in the different parts from the first tints to green—and superposed plates do not—that will shew that there is cohesion of parts and continuity in the first plate and not merely a layer of fine particles.

14279. Gold precipitated by sul. iron gives I think screens of different colours at different times. If this is not due to addition of the effect of the persulphate of iron produced, it is probably due to the different sizes of the particles. As these particles are probably less in thickness than the thickness of a transparent plate of gold, they ought to transmit coloured light; and as they are separate the one from the other, the light through a greater number of them ought not to be changed in colour though it should be diminished in quantity.

14280. Let a precipitate settle: does the colour of the *column* then vary in different parts, as particles of different sizes arrange themselves at different depths. Use a rectangular column so as to compare width in one direction with breadth in another part and direction.

14281. Shall probably be able to deposit a film of silver, copper or other metals by Electricity on a *transparent* film of gold, using cyanides and a light beneath during the process. In this way may examine such metals.

14282. When forming such plates, especially of the more oxidizable metals as lead, tin, etc., keep them under the fluid and examine them in it. Use dilute alcohol to wet the metals.

14283. Perhaps obtain crystals of some of the metals thus and examine them by polarized light.

14284. *Transparent crystd. lead*—see notes 783, 4, 1935, Decr. 1833.

14285. *Zinc crystals*, 1337, Jany. 1834—perhaps obtained on plate of Gold.

14286. *Transparent silver*, 1448, 9—Jany. 1834.

14287. On reducing Acetate of lead by Voltaic battery, I obtained in the old time very good crystals of lead, quite transparent apparently. May most likely obtain other metals so by the same means. Do the crystals of lead polarize or depolarize light?

14288. Reduce by battery Silver—Platina—Gold—Zinc—Palladium—copper—iron—antimony—bismuth.

14289. The sensible lustre of the films depends much on the character of the surface on which they are laid. If that be polished they are polished. If that be dull they are dull, because the film adapts itself to every minute variation of form in the surface. See the films on dull and polished cards and magnify the former—then the lustre came forth.

14290. List of numbered films—p. 2990, 3004[1].

1 to 42, films of gold, 14671, 6.
1 to 23, „ „ increasing in thickness, 14671, 6.
8, 11, 12, 15, „ „ graduated or sliding thicknesses, 15238.
17, very dark[2].
23, beaten gold leaf.
27, has been heated.
28, in Canada balsam.
29–33, on polished card.
34, on dull unpolished card.
35–39, two films superposed, 14671.
40, 41, three films superposed, 14625.
42. Four films superposed, 14625.
43–46. Palladium, 14622, 72, 5239.
47–49. Silver, 14672.
50, silvered tube in halves.
51. Thirteen superposed films of Gold, 14625.
No. 4, broken into 2 pieces, 5 Feby.—by a fall.
52. Film, Silver from Nitrate by phosphorus vapour.
53. Film, Silver from Ammoniacal Solution by phosphorus vapour.
54. Gold precipitated by Sul. Iron (14353), 14640, 70.
55, 56, 57, 58, 58*b*. Thick gold films by phosphorus (14352, 4, 8), 14671.
59. Silver by Electy. on zinc, N.A., etc. (14375).
60. Do. . . . . (14375).
61. Gold Do. . . . . (14381).
62. Do. . . . . (14384).
63. Do. . . . . on paper (14385).
64. Do. . . . . on glass (14386).
65. Do. . on one side . Do. (14415), 621, 72.
66. Do. . two films . . Do. (14416), 621.
67. Deep gold on sol. chlorine—residue (14420).
68–73. Do. . progressive . . (14420), 14628, 69, 5237.
74–82. Extra deep gold. Do. . . (14421), 14620, 69.
83. Deep gold thinned by M.A. . . (14432), 620, 69.
84. Film from red fluid by Evaporation (14437), 627.

[1] See pp. 55 and 67.
[2] This entry is struck out and "made 96?" is written against it in pencil. See par. 14493.

85. De la Rue's film of Platinum . . 14622.
86–92. High gold subjected to different fluids (14441).
93. Leaf Gold heated (14486), 624, 745.
94. White Gold heated (14487), 624, 73.
95. Purple gold film heated (14488)—also pressed (14529[1]), 14566, 626, 74.
List continued at page 2990[2].

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14291. Prepared a standard weak solution of Gold marked G.

14292. Prepared a standard solution of proto sulphate of iron, I—it consisted of 1 vol. saturated solution at 54° F. plus 2 vols. water, and a little sulphuric acid to keep all in solution during the changes.

14293. Certain precipitates of gold were then made thus:
No. I was 1 vol. G + 7 vols. water + 1 vol. I; it gave an immediate good precipitate of gold through which a candle flame could be seen in daylight. 7 vols. more of water were added, then the tint of the precipitate was weak. More sol. I was added but there was no more effect—all the Gold was thrown down.

14294. No. II. 2 fluid drams of G made up by water to 10 drams: 2 fluid drams of I made up by water to 10 drams. The two mixed—an immediate precipitate. We know from No. I there must have been enough iron solution here.

14295. No. III. 2 fluid drams of G made up to 10 drams: $\frac{2}{2}$ dram of I made to 10 drams—mixture—an immediate precipitate like the former.

14296. No. IV. 2 f. drams of G made 10 drams: $\frac{2}{3}$ dram of I made 10 drams—mixture—precipitation as before.

14297. No. V. 2 f. drams of G made 10 drams—$\frac{2}{4}$ dram of I made 10 drams—mixture and precipitation as before.

14298. No. VI. 2 f. drams G made 10 drams—$\frac{2}{5}$ dram of I made 10 drams—mixture—precipitation as before at once.

14299. No. VII. 2 f. drams G made 10 drams—$\frac{2}{10}$ dram of I made 10 drams—mixture—precipitation as before at once.

14300. When these were looked at by candle light they seemed as nearly alike as possible—there appeared to be no want of sul. iron in No. VII—tints appeared to be all alike—left until the morrow.

[1] ? Par. 14529*b*.

[2] See p. 55.

14301. Simple sun light, bright, sent through gold leaf on to white paper, transmitted green rays on to the paper in the favourable parts. When gold near up to the paper, then the light on the latter was yellow from light *reflected* by the corrugated under surface of the gold on to the paper, that light originating at the white paper round about: but when that was cut off or when the gold and paper was separated, that yellow light in a great measure disappeared.

14302. Must be careful to cut off all such and all outside light. Must also observe in a dark box.

14303. When the films on glass were employed, the transmitted light on the paper was violet or purple.

14304. When the gold film was 12 inches or more from the paper screen, the form of the coloured space was perfectly distinct. Also a pencil point, etc. held near the gold film on the sun side gave as perfect a shadow on the paper as if it had only gone through glass.

14305. Must make this experiment in dark box and with three or four superposed films—also with intense light. Seems to indicate continuity of the plate.

14306. Obtained a solar spectrum—threw its rays successively on to beaten gold leaf—the green went through best and appeared on the paper. Use a dark box.

14307. Spectrum rays sent through the films—all the colours passed through the thinnest films—and some of all through most of them.

14308. There is always extinction of the rays besides reflexion and transmission—the very thinnest give shadow and colour. In the thinnest films the ray transmitted is so coloured as to suggest that such as have not gone on have lost more by absorption in the gold than by reflexion backwards.

14309. Mr De la Rue tells me that beaten gold leaf inclined at an angle of 45° to the course of a polarized ray depolarizes—consequent upon the strain of the forces applied in the heating. When perpendicular to the ray it does not depolarize. This polar[iz]ing effect is an argument in favour of the continuity of the mass.

14310. Glasses of precipitated gold of last night (14293, etc.): this morning the Gold has settled against the sides and bottom in all of them.

14311. The separation of Gold is perfect in all—apparently.

14312. Placed in a row and looked into from above, the sides opposite to the light looked yellowish because of the light reflected to the eye by particles of gold adhering to the side; the bottom looked dark and purplish or violet, because of the deposit there.

14313. The yellow reflexion was stronger in some than in other[s]—the order: *Strongest* 7, 6, 1, 2, 4, 5, 3 *weakest*. On looking down there was also a difference, the order being thus—*darkest* 3, 2, 4, 1, 5, 6, 7 *lightest*—so that they are nearly the reverse of each other, as they should be: the effect depending on the quantity adhering to the side or falling to the bottom.

14314. When the glasses were inclined so as to be parallel to the general rays of light and were looked into from above, all looked yellow at the bottom from reflexion of the light by the gold. But when looked at from below, i.e. by transmitted light, all looked violet or purple, and all of the same tint of colour—no approach to green.

14315. Glasses I and II were stirred up and part of the deposit so diffuzed through the fluid. The obscurity was not nearly so great as when the precipitates were first made, but the tint was generally the same as before. Some of the gold adhered to the glass and some of the particles seemed to have adhered together, forming larger particles. The precipitate soon fell again and in half an hour or less was nearly all on the bottom.

14316. The fluid was poured off from the other deposits and more sul. iron added to it—all remained unchanged; no further separation of gold took place—so plenty of iron even where least, as in No. VII (14299).

14317. The deposits being put together and agitated with part of the fluid, made a very obscure mixture—still the colour was the same as before. It was foggy and did not transmit a clear image of an object through it—particles were of course not continuous. The gold began to fall at once and in two minutes a part near the top was clear, but though the light transmitted was

different in quantity in different parts—the colour was the same—the heavier and the lighter seemed to give the same tint and no green any where.

14318. The aggregating power of the gold seems very considerable. Collecting all the precipitates into one glass and shaking this up from time to time, the particles cohere—the opacity of the fluid becomes less and less and the particles sink more and more quickly. Also when a splinter of wood was used to disturb that which adhered to the glass, the end came out gilt at the rubbing places with patches of bright yellow gold, apparently continuous, being violet black w[h]ere the unrubbed particles were. Gold probably welded by rubbing. Pressure makes gold powder adhere.

14319. *Evening.* Have been this morning to De la Rue's to learn his mode of making the films of Gold—is as follows. A piece of phosphorus about this size is dissolved in about 30 minims of Sulphuret of carbon to form one fluid P—a solution of Gold free from acid and containing about a sovereign in 2 or 3 ounce volumes forms the second fluid G. A clean plate of flat glass about 5 inches square—a glass capsule 6 inches in diameter—a large Wedgewood's dish holding 3 or 4 quarts of water—some strips of flat glass—card board, polished copper, etc. A little of the phosphorus solution P was poured into the glass capsule and moved over its surface to distribute the phosphorus—most of the sulphuret of carbon evaporated. A portion of the gold solution G was poured on to the *clean* glass plate—spread over its surface by a glass rod—the excess quickly poured off at one corner back into the bottle, and then the wetted plate inverted and placed over the phosphorus in its capsule; gradually a film of gold formed which could be recognized by reflected light because of its colour and appearance. Then the glass plate was turned up and, being brought over the dish of water, was inclined a little to the horizon and depressed until one edge and gradually the whole was under water—the metallic film floats; if well made it is stiff and does not change in form by the surface currents of the water—if badly made it moves about as a film of oil on water, there being little or no cohesion of the parts. A piece of glass or of card being immersed in the water—brought beneath the

film and raised, brings up the film with it, and then bibulous paper can be used to dry up the water and leave the film on the glass.

14320. To-day he had a fresh solution of Gold and we found it exceedingly difficult to form the films—they formed imperfectly or slowly, and when removed would not cohere. The solution G was evaporated to drive off acid—part had a little ammonia added and was then filtered, but the films did not form. There had been something favourable in the former solution.

14321. A very fine red fluid is obtained by letting pieces of the evaporated phosphorus, with adhering Sulphuret of carbon, lie at the bottom of a pan containing a very weak solution of gold—the mere washing.

14322. A piece of copper on which a film of gold had been taken up was held over the mouth of a bottle containing sol. Sul. hydrogen—there was instant [action] of the vapour on the copper under the gold film. I fear the films are very porous and cannot in any optical sense be considered as continuous plates.

14323. I suspect that the difficulty to-day of obtaining the films has been due to the formation of *a continuous film*, and the impossibility of increasing the thickness of such a film by continuing the action, inasmuch as it cuts off the connexion between the solution on one side of the film and the phosphorus vapour on the other. It is true that visible films of the former kind ( ) were produced and could be put on to the water, but then the parts moved about. I think that the visible parts are due to breaking of the first film formed by the motion of the fluid on the plate, and that as these breakages occur, one part thickens and then a true continuous film forms and stops the action. That the whole should seem to move on water is quite natural, for the strength of the one continuous film could never stand against the mechanical forces there employed.

14324. So still hopes of a continuous film by one means or another.

14325. Must ascertain if these films conduct. If they do, then may hope to employ them as skeletons upon which to deposit by Electrotyping a film of continuous gold.

14326. Very fine particles (much within the length of an undulation) may not disperse by refraction (or inflection):—if all particles

dispersed, we might use the effect as a test of continuity, for an interrupted film would disperse but a continuous film not.

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14327. Send voltaic light ray through purple film of gold and observe the shape of the general shadow, and also the shape of objects casting shadow placed between the film and the light, as a dissolving view glass, etc. Use also plates with deposits of real particles—as particles of gold settling on to a plate—or of prussian blue, etc.

14328. Remember the red bunting effect at the Trinity house.

14329. May probably prepare a thin film of Gold by Electro-typing on silver or platina—and may examine some of its properties on them—but the metal beneath is almost sure to interfere with the optical characters. Perhaps might contrive to dissolve off the silver by pure nitric acid and leave the Gold film, floating and yet continuous. Try with a film increasing in thickness from part to part.

14330. As to incoherent particles of a film of gold and the want in such cases of continuity—as also of the impossibility that any microscope can reach such cases when they may well exist, we may think of an animalcule and its microscopic measurement, yet the smallest of them has myriads of particles which are continuous in all the various conditions required by organization.

14331. There seems to be no better or closer chance of testing continuity than by applying gases and vapours and observing their power of permeating the films.

14332. The vibrations of light are so large compared to many of the dimensions that occur in thin plates and particles that, if the phenomena of reflexion and transmission depend on them, they ought to change much with particles and films of these dimensions. Do they depend at all upon other considerations, which still have relation to dimension?

14333. There may be particles so small as to have no power of refraction. Refraction may depend essentially on mass. Vibrations may perhaps (as they are so large) become shorter as they go out of one body into another, and return again to their first dimension as they issue out—or change to another vibration.

14334. Have made different precipitates (14293) varying in the proportion of Gold:

No. VIII. $\frac{1}{10}$ f. dram of G made by water 10 drams–2 f. drams I made 10 drams–mixture–precipitation at once.

No. IX. $\frac{1}{4}$ f. dram G made 10 drams–2 f. drams I made 10 drams–mixture–immediate precipitation.

No. X. $\frac{1}{2}$ f. dram G made 10 drams–2 f. drams I made 10 drams–mixture–precipitation at once.

No. XI. 1 dram G made 10 drams–2 f. drams I made 10 drams–mixture–precipitation.

No. XII. 2 f. drams G made 10 drams–2 f. drams I made 10 drams–mixture–precipitation at once.

No. XIII. 4 f. drams G made 10 drams–2 f. drams I made 10 drams–mixture–precipitation at once.

No. XIV. 6 f. drams G made 10 drams–2 f. drams I made 10 drams–mixture–precipitation at once.

14335. These were mixed at 2 h. 30 m. Character of tint nearly alike in all and same as before–the light transmitted a purple or purple blue or violet. Reflected light a brown gold tint. By 4 o'clk., all the precipitates had sunk a little, so that the top of all was clear, but the depth cleared increased from VIII to XIV–in VIII it was just at the surface–in XIV half an inch was clear–far more gold had descended (in proportion) in XIV, XIII, XII, etc. than in VIII, or IX, as if the particles in the former were larger and heavier, as if all not alike–or as if those that touched had cohered.

14336. The recent precipitates were more diffused than the old ones of a day or two–as if aggregation had taken place among those that touched each other.

14337. The precipitating glass of mixture had the purple tint on its side, by film or particles of gold.

14338. The precipitation of the gold appears to go on with increasing rapidity, i.e. it seems to settle much quicker thrgh. the lower part of the fluid than the upper–as if the particles ran together or cohered and so became heavier–as rain drops do.

14339. Put 3 f. drams of G to 32 drams of water and 6 dr. of I to 32 of water; mixed the two, making a precipitate like IV or XI; put it into a square cell and a plate of glass at the bottom for the precipitate to settle on–left it for the morrow (14353).

14340. This morning the seven glasses VIII–XIV (14344[1]) had all deposited to the bottom–the deposits looked through had all the purple or violet tint. On pouring off the supernatent liquor, even the one from most gold had plenty of iron, for further addition of it caused no precipitate.

14341. The deposits differed in their adhesion to the bottom of the glass: the larger deposits of XIV, XIII, etc. were easily disturbed and broken up–but the deposits of VIII, IX, etc. adhered well to the bottom. On pouring off the fluid and adding water to wash the gold, the addition seemed to favour the adhesion of the gold to the glass–and perhaps also that of the particles one to another–for the tint and its depth appeared rapidly to diminish, and by comparison with the obscurity on the first precipitation one would have supposed that $\frac{3}{4}$ of the gold or more had been taken away.

14342. The red fluid (14321) made 6 Feby. does not apparently settle; is uniformly red. A portion passed thrgh. a filter twice went through red, but also left a stain on the filter shewing the separation of some of the particles.

14343. Seven specimens were prepared (14355). No. 1 an unaltered sample, XX; No. 2 boiled, XXI–the tint became lighter and the solution turbid from the aggregation of the gold particles, which then conferred a yellow brown tint, the solution or liquor being rose colour–more boiling threw down or aggregated most of the gold (14422).

No. 3, dilute S.A. added–colour changed at once from rose to violet or blue.

No. 4. Mur. acid added–same change.

No. 5. Nitric acid added–same change.

No. 6. Little ammonia added–rendered the fluid purple–appears to keep the particles nearly in the original state.

No. 7. Little potassa added–changed to purple and then violet or blue, looking like 1, 2 or 3.

14344. A little of 4 and 5 put together did not cause solution of the Gold or change of colour.

14345. A little of No. 3, by addition of ammonia, did not return to the first tint but took up 6 tint.

[1] ? 14334.

14346. Five hours afterwards. 1 was still uniform and unchanged. 2. Still uniform, rose colour. 3. Settling from the top—purple. 4. Settling—purple. 5. Settling purple. 6. Settling violet—7. Hardly settling (14355).

14347. A glass full of the deep red fluid (14342) was poured from off the phosphorus dish last night—this morning there was a superficial film of bright gold, which being taken off on glass and dried, was bright yellow gold by reflected light, darkish by transmitted light, but then seemed mottled and full of holes. Examined by the microscope, was found to be a mere webb—the holes being far more in surface than the solid parts or reticulations.

14348. At the bottom of the glass was a dark black or blue black deposit of gold—it was aggregated or brown gold; being diffused through water, it did not give the deep red tint, being too much aggregated.

14349. The fluid was clear and had very little colour—it was of a pale brown tint—and contained but little gold. It seems that when this red solution is too concentrated as to gold, the particles cohere and precipitate.

14350. In the phosphorus dish was another portion of fluid with the same floating film of gold and like characters. The liquor was red and like the former. By heat and boiling, all the gold could be separated and thrown down.

14351. The red fluid in chlorine water—rapid action—the red colour disappears and an ordinary solution of gold is formed—which by proto muriate of tin gives the usual gold characters.

14352. For Gold films as described (14319). A solution of phosphorus in Sulphuret of carbon was prepared—also a glass dish for its evaporation—and a covering glass plate for the gold solution. This was divided into 4 parts and the surface in each division moistened with solution of Gold:

No. 1 with De la Rue's solution (14320),
No. 2 ditto. much diluted,
No. 3 ditto. more diluted,
No. 3[1] ditto. still more diluted,

and then the plate was turned over the phosphorus dish and left at $11^h\ 20^m$. It soon began to shew feeble signs of film—at $4^h\ 30^m$,

[1] Evidently a mistake for 4.

No. 1 was dark—No. 2 less dark, No. 3 scarcely sensible—No. 4 still less sensible. All was left until the morrow (14354). Nos. 55, 56, 57, 58. Results.

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14353. Took out the glass plate (14339) with precipitated gold on it—could not wash it but inclined it and dried it as much as I could by bibulous paper—then let it dry. In drying, the surface, which at first had no power of reflecting gold tint, acquired that power, i.e. presented a golden appearance, looking like dead brown metallic gold. This was simply a consequence of the removal of the water and of the effect of its refractive power. If wetted, it again became unmetallic. There is saline matter mixed with this gold and that appears in the crystalline films. The gold particles are very loose and easily disturbed. When looked through, this specimen is black or opaque—no green here—it shall be numbered 54 (14290, 14403).

14354. The four films (14351) were all damp but the moisture not flowing. No. 1. Poor—broken up—golden by reflected light—purplish by transmitted light—but poor—a drop or two of water added, the film broke up, having no cohesion—the fluid tested by proto mur. tin shewed no signs of gold. No. 2. Poor—a little golden reflexion, but very little—purplish by transmitted light—film much broken up. Water broke up the film to a purple or violet powder. No gold left in solution. No. 3. The fluid had run together into a drop and there was golden reflexion and good purple by transmitted light, but the place was small. Water broke up the film except at part where the drop had been. No gold left in solution. No. 4. Scarcely any sensible film—no effect by Mur. tin—no gold in solution. No useful result by employing much diluted solutions, and all tends to shew the films are porous.

14355. The seven glasses (14343) of yesterday were examined. No. 1 shewed no signs of settling or of change—colour was as before. No. 2 is settling—the top is clear—it has the same fine rosy golden hue as before—particles must be exceedingly small. No. 3 had settled perfectly—liquor clear—deposit violet—the colour but small in effect compared to that of the original solution, before acid added. No. 4 is as 3, settled perfectly. No. 5 is as 3.

No. 6 has also settled (ammonia) but the deposit has a beautiful violet colour, very different to the others—when the solution was poured off and the deposit stirred up in pure water, the particles adhered together forming larger particles, distinctly separate from each other and giving a poor colour on the whole. As to colour, I think that this and No. 1 shew well the effect of aggregation in diminishing the colour. No. 7 as 3. Nos. 3, 4, 5 and 7 were dismissed but Nos. 1, 2 and 6 were left until Monday (14360).

14356. Some copper plates from De la Rue, with his films on them, put into an atmosphere, weak, of sul. hydrogen—the gas acted on the copper under the plates, appearing to be transmitted. There was variety of colour produced. Must put some films on platinum to compare the actions there with the action on copper (14361).

14357. *Phosphorus films.* Put several separate drops of De la Rue's strong solution of gold (14320) on a glass plate over phosphorus (14319) at $11^h\ 40^m$. At $12^h\ 25'$—beautiful films covered the downward surface—golden in parts but evidently not continuous, for they have contracted from the edges and have split and evidently have causes of motion in the circumstances either by force of clinging to each other or otherwise. That which covered the surface of the whole drop at one time is not large enough to cover it at a later period, so contracts and fresh film forms at the edges. At $1^h\ 15^m$ (in 95 minutes) the films were very golden and beautiful (as seen thrgh. glass and fluid, for all was left undisturbed) but breaking up by cracks across and at the edges. At $4^h\ 15^m$ ($6^h\ 35^m$ in all[1]) they were very beautiful—very bright and golden and now wrinkling up. The process still goes on and the films thickness, but this can hardly occur without a porosity of the films. At 8 o'clk. (i.e. after $8\frac{1}{2}$ hours)—the films were removed—plenty of gold was found in the solutions between the film and the glass. This was washed away and the gold films washed in water. They were strong; sank freely in water if wetted on both sides, though floating if wetted on one side only. They were very bright and golden. They were put upon glass—the water partly removed by bibulous paper and then dried. When on the glass, they looked very bright at first, after the water was

[1] ? $4^h\ 35^m$.

sopped away; then as they dried a dull appearance came over them and when quite dry, after a time, the brightness returned again. Was that due to porosity and water in the pores, or to a little phosphorus, or what?

14358. These thick films are very golden on the glass. I cannot see ordinary light through them or get any signs of transparency, but they will require mounting and examination in a strong light (14403).

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14359. Two or three of the film spangles (14358) were put into water and set aside.

14360. The three glasses of Saturday examined (14355). No. 1 as before is unchanged. No. 2, the aggregated gold is settling and the clear liquor above is of the same colour as No. 1 but much weaker. On stirring up the settling particles, all became lustrous as before, by the mixture of colour and metallic reflexion. The lustrous particles seem to be exceedingly fine and not therefore to owe their lustre to their aggregation—is the purple or red tint due to particles of paramagnetic gold, or is it a real solution? No. 3, flocculi of considerable size had settled here, but they were of the same fine violet colour as before—looks much like an insoluble state of that which in solution produces No. 1.

14361. The copper plates (14356) have fine colours where the films have been and they do not wash off by wetting and soft rubbing.

14362. Proceeded to work with Electricity, endeavouring to deposit thin films of metals, and first silver for trials.

14363. Battery—2 pr. of Grove's plates—the usual Sulc. acid in the outer cell but only water acidulated with N.A. in the inner cell.

14364. There are four solutions of silver in Cyanide of potassium before me. That marked 1, from the shelves—a drop of it on a silver plate connected with the Pos. end of the battery; a clean plate of copper, made Negative, brought up to the top of the drop and retained in the circuit whilst deliberately counting six—copper removed—washed—wiped, which removed a little loose silver—the copper found silvered but brightest at the edges of the

wetting—reddish in the middle of the drop—but this only from loose or divided silver, for when rubbed all became white.

14365. Made like contact on the same place—counting 20—better silvering—but it looked coppery in the middle—nothing wiped off—and rubbing made all white. There was a separated white flocculent cyanide in the solution—in fact the cyanide formed at the Pos. pole had not dissolved—and so the drop of solution had become weakened in silver.

14366. Another solution from the shelves, marked 2—gave results like those of solution 1—but for the same time, stronger effects—it is probably a stronger solution.

14367. A third solution from the shelves, marked 3—counting only six gave a strong silvering and I think less wiped off after the first application than with 1 and 2—all the part of the drop spot was white—no red in the middle. Merely putting into contact with the fluid and out again at once gave a good coat of silver—all white, as if little or no loose silver.

14368. A fourth solution of silver, No. 4, made by precipitating cyanide from the nitrate—washing it well and dissolving it in solution of cyanide of potassium—gave on using copper as negative—with one quick touch, good silvering, a little dull in the middle from want of aggregation—by six counts an excellent silvering—by 20 counts a famous silvg. but red or dark at place of most intense action. When these dark places are polished by rubbing, the[y] pass into well silvered surfaces.

14369. Cyanide of silver is formed at and adheres to the Pos. pole, which is of silver, so the solution becomes thinned of silver and at last gas is evolved against the Neg. copper pole. To prevent this, a sufficiently large quantity of the solution must be used.

14370. Nos. 3 and 4 seem to be the best solutions, and probably a weaker battery and more time would be better with this copper N. pole.

14371. Now employed a *Zinc N. Pole*, the object being to dissolve it away at last and leave the silver film by itself. Used zinc foil, cleaned by whitening and by fine emery and a cork, and the solution 3 (14367). The zinc alone acts directly on this solution, precipitating a film which is at first reddish and then black, and is evidently unaggregated silver.

14372. When the Zinc was made Neg. and then placed for a moment in contact with the drop of solution—and instantly washed—a brown film was formed—being wiped and touched with the solution an instant longer, a *yellow* tint was produced—*a golden tint*—wiped and touched again, this yellow film passed gradually to a cream silver tint.

14373. When the zinc, not Neg. but per se, is in contact with the solution, the same changes occur—the appearances are successively yellowish—brownish—creamy—silvery; and so it goes on, but the coat of silver is not bright. When the Zinc is made Neg. and then put in contact, the same changes occur.

14374. When each successive film is rubbed and so laid together, the coats are whiter and the whole result more continuous apparently.

14375. A dilute Nitric acid—about 30 drops pure acid to about 2 oz. of water—was put into a plate. Any of the films described, being laid on this acid with the zinc face downward, float, and the zinc is gradually dissolved and the silver left as a film, which if thick, holds together as a film—if thin, generally breaks up or runs together as a blackish film. For the acid evolves an electrical action between the zinc and silver, gas is evolved and especially at the edge and between the separating zinc and silver, and this mechanical action sadly knocks the film about. The zinc is gone in two or three minutes, too quick for a good result. The acid scarcely acts chemically on the silver film at all.

No. 59, three films of silver prepared this way (14403)

60, silver film thrown down by action of zinc foil directly on the solution of cyanide—continued (14403).

14376. Now worked with a gold solution in cyanide from off the shelf, having a gold Positive Electrode, and made the Negative electrode of silver. When silver unconnected was put into the gold solution, it caused no precipitate whatever.

14377. Made the silver Negative in the solution at a distance from the P. electrode for 8 or 10 seconds. At the nearest part there was a yellow golden film, at the further weaker part the effect was insensible, and between these there were gradations—at one part the tint was violet or purple—but on washing and wiping, that disappeared, as if due to disintegrated gold, and then all

became gradual progression from one part to another. The silver plate was then put in for three minutes*, with a large surface varying in distance from the Pos. gold; it was then removed and washed. There was an effect all over, but feeble by reason of the extent of surface—the tint was yellowish and the whole feebly gilt—there was no dark or purple part.

14378. By employing a smaller closer surface of silver, I quickly threw down a film of which the nearer part was brown gold—dull—but by rubbing becoming good gold polish—the more distant part was yellow and polished; the deposit there had not gone on to the evident production of loose gold. When the gilt part was polished and yellow, still on making it negative in the solution, brown gold was deposited on it again, as if the process was too quick.

14379. Probable that a stronger solution—a weaker battery—and more time would give a better result; or the same battery and more distance in the fluid.

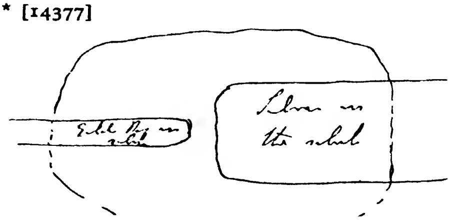
14380. Proceeded to work with Negative zinc (14371, 14377) and the gold solution. The zinc foil alone in the solution soon threw down a dark purple coat, which by washing and wiping came to a dull purple gold lustre. But on making the Zinc Negative and then putting it in contact with the gold solution, the zinc seemed to gild beautifully; it was yellow at the first instant and bright as gold. Being gilt on both sides, the end was cut off and put into the dilute Nitric acid (14375) and submerged. Action occurred and gas appeared in bubbles over all the surface, as if the acid went through, which probably it did (   ).

14381. When the Zinc was dissolved, the double film was transferred to water and examined by the eye glass—it was evidently much crumpled up—but very golden in appearance. It was transferred on to a glass plate safely, and I think is transparent and green (   ). No. 61 (14403). It promises well.

14382. If the zinc were perfectly polished so as to present no irregularities in thickness, perhaps might get a continuous crust on the metal—but then it will be difficult to get it of[f]. Perhaps lay the gold zinc on glass plate, and dissolve by acid *over* it.

14383. The yellow of the gold on the zinc very striking.

* [14377]



14384. Zinc made Negative brought against the gold solution on one side only and near the Positive electrode. Came away brown, which by soft wiping became bright—the force had been two [? too] strong. This on dilute N. Acid with the Zinc downward—when zinc dissolved, was transferred to water and at last on to glass. It was thicker than the former films—seemed purple or slate coloured all through—had a bright gold colour. No. 62 (14384).

14385. A face of Zinc made Neg. for an instant only in contact with the Gold solution—out—washed—was of good yellow colour, more than I should expect for gold only if zinc or metal were not beneath. Perhaps the plate of gold is thicker than is expected, the Zinc adding its momentary power to that of the battery. When put on to the dil. N. Acid zinc downwards, one edge became wetted above; as the solution went on, this had a better appearance than the other parts, for they seemed to tarnish and lose colour; but perhaps this was due to mechanical breaking up between the gold and zinc. As the solution went on, the film was too thin to hold together—it started apart, but parts reflected metallically and they looked more like platinum or lead than gold—not yellow as gold or like gold. The transmitted light was dark slate, not green—the film more like De la Rue's films in this respect. Part was transferred to water and then to paper. No. 63 (14385).

14386. Made another weak or thin film of gold on zinc (14385). Placed dilute N.A. on a glass plate and the zinc on it, so as to dispense with transference of the gold film. Had to move it about to bring the zinc and acid in contact—then washed by removing the acid and adding water several times; at last removed the water and left the film, in many broken fragments, adhering to the plate. No. 64 (14386).

14387. A like gilt zinc put into the dish of dilute N.A., beneath the surface and with the gold downwards. It dissolved much as the last did—perhaps fewer gas bubbles, but the gold film left was all crumpled up and of a dark slate colour.

14388. The action of the dil. N. A. on the ungilt Zinc is slow by comparison with that on the gilt zinc, the local galvanic action due to the gold being away. The zinc dissolves almost entirely but leaves a little black residue floating—copper, etc.

14389. Copper foil does not replace zinc foil well. When alone in the dilute N. Acid, it was not sensibly acted on. When the copper foil was alone in the gold solution, it did not sensibly act on it. Being made Negative, it acted and a film of gold formed on it, apparently in excellent state, i.e. the gilding seemed excellent. Placed this plate on a mixture of 1 N. Acid and 2 water—the copper dissolved but very slowly. On using a still stronger acid, the copper is acted on, but the gold peals off from it in a crumpled film by reason of the powerful voltaic action which occurs at the angle of contact and which runs up beneath the two metals like a knife. Very Natural.

14390. Tried to thicken the De la Rue film by making it negative in the gold solution, and for that purpose employed the fragments (two) of the broken glass No. 4 (14290). Did not succeed, but my attempts to make contact with it were rough.

14391. This De la Rue film dissolved slowly in the cyanide of gold, it not being in contact with the poles of the battery. As it dissolved, from purple violet it became blue violet, changing much in colour—probably due to particles being made smaller. May use this solution probably to thin gold leaf, and perhaps also a like solution to thin silver leaf and other metals.

14392. Proceeded to act on *silver* and *gold* leaf by an atmosphere of *chlorine*, in hopes of thinning the metal plates and observing them as they became thinner. Silver leaf hung to the sides of a dry bottle, and then some chlorine gas poured in, perhaps $\frac{1}{3}$ of an atmosphere. The silver instantly acted upon and gradually transmitted light—finally it became like a thin translucent horn. As the effect proceeded, the appearances by reflected light changed, the full metallic lustre disappearing instantly—but imperfect lustre with much mother of pearl tints and appearance came on, until all became like thin horn. As to the transmitted light—the change was too rapid to observe that well, but some parts which were more protected by surrounding metal and otherways presented a beautiful series of colour, purple, rose, etc., gradually passing over the part as the metal became thinner. Must observe all this again, for there are some good effects here.

14393. *Gold leaf* in chlorine dissolved instantly, leaving a moist film of chloride of gold. Must try this in a very dilute atmosphere.

14394. *Silver leaf* laid on an aqueous solution of chlorine gradually becomes a floating leaf of chloride. Appearances do not seem to be very instructive, but must look at it again.

14395. Gold leaf on an aqueous solution of chlorine gave a very beautiful result. The solution was in a glass dish, placed on a plane reflector and covered by a glass plate, so that the metal could be observed both by reflected and transmitted light. The leaf gradually became thinner and thinner until a mere ghost of a leaf was left. By reflexion—when thinned to a certain degree, the light and colour diminished, becoming less and less until scarcely any light was reflected, the film preserving its perfect continuity all the time and keeping both place and shape. At the same time, more and more light passed through and this was always green: when the film was so thin that light reflected upwards and passing only once thrgh. the film had scarcely any colour, still, by observing a ray which passed through the gold to the reflector and then upwds. through the gold again, so as to go thrgh. it twice—the light was seen to be well green—always the same tint (14397). The reflected light also was either yellow or colourless. The process is excellent and the films may easily be thinned, washed and preserved on glass.

14396. A leaf of gold is 3·25 inches square. So 13 leaves of pale gold = 137·3 square inches; and being collected by Alcohol, dried and weighed, they came to 1·6 grains. So a square inch of this gold equals 0·0116 of a grain. If an inch cube of gold weighs 4898·5 grains, then the leaf is $\frac{1}{422,284}$ of an inch in thickness (14424, 89, 92). Doubtful[1].

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14397. Resumed the dish of last night (14395); the ghost of the gold leaf was there on the solution of chlorine—took it up on a glass plate—it was a very thin transparent film and proved to be chloride of silver, derived from silver in the leaf, which was of pale gold. This chloride it was that kept the leaf so well together. The gold seemed to transmit green light; but pale gold transmits blue. The green probably was produced by the transmission of

[1] A pencilled comment.

the ray through the yellow solution of chlorine between the gold and the mirror beneath (14395).

14398. Tried the solvent power of solution of cyanide of potassium over gold, silver and other metals. Have a strong solution made two days ago and now brown in colour. A piece of silver leaf laid on its surface dissolved instantly—a piece of pale gold leaf laid on the surface dissolved more slowly, but very soon. Both metals in dissolving broke up when very thin, the films cracking and opening out with a little rend or repulsion.

14399. 1 Vol. of the above strong solution and 7 vols. Water made a solution which dissolved silver leaf quickly—added 8 vols. more of water so that the strong solution only $\frac{1}{16}$th part. Same solvent power over silver laid on the surface. When the silver leaf, instead of being laid on the surface, was submerged and wetted on both sides, it then dissolved very slowly, requiring 10 or 20 times as long as when on the upper surface. There is every appearance of the air having some action.

14400. *Pale Gold leaf* laid on the surface of the weak solution of cyanide (14399). It dissolved freely, breaking up mechanically when very thin. Again, some put on the surface and some *under* the surface; that on the surface dissolved freely—that below remained a long while undissolved.

14401. *Brass leaf* on a drop of the strong solution (14398) soon dissolved away.

14402. A piece of *Zinc foil* on the top of another portion of the strong solution—appears to dissolve, but is thick and was left covered.

14403. Examined some of the deposits and films. No. 54 (14353), deposit of Gold by Sul. Iron, is just a powder—no transparency or colour. No. 59 (14375), Silver by Electricity, is full of holes, but the thinnest parts seem to have a dark brown transparency. No. 60 (14373 [1]), Silver by Electricity, is opaque. No. 61 (14381), Gold by Electricity—a double film; where single it is transparent a little, the passing light being slate coloured. No. 62 (14384), Gold by Electricity, transparent in the thinnest places, where single—transmitted light is slate colour. No. 63 (14385), thin electric gold on paper, very little light through. No. 64 (14386),

[1] ? 14375.

thin Gold by electricity on glass—translucent—blue or slate—has gold reflexion. Nos. 55, 56, 57, 58 (14358), the thick spangles by phosphorus.

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14404. Yesterday pieces of Silver leaf and of pale gold were laid upon glass first moistened with white hard varnish diluted with Alcohol. To-day they are pretty dry—the adhesion was very imperfect at the wrinkles, puckerings, etc.

14405. Some of these pieces of Silver leaf were placed in a weak atmosphere of chlorine—they quickly changed and were not long becoming nearly all chloride of silver. Being watched by transmitted light, the progression was from darkness to dark slate colour—green in parts—green brown—brown—light brown, i.e. the dull white of the fully made chloride. Another set in a still weaker atmosphere of chlorine suffered quick action—the progression was from black opacity by slate colour to the final dull state—presenting no green. Whether the green is a true silver colour or not is not yet settled.

14406. By reflected light the film of chloride over the silver beneath gives green, red, etc. in different positions, acting as a thin plate to the light passing through. This colour must shew, I think, that the silver reflector beneath is continuous, i.e. that the chlorine does act as a thinner of the plate of metal in the first instance and not a disaggregator of it.

14407. Would a metallic surface made up of particles, like de la Rue's films, reflect light so as to give colours of thin plates? Perhaps may find a test here of continuity. If particles not having continuity can reflect light as a continuous surface, will it not shew that several separated particles may act at once and therefore at a distance on a ray of light—and then must not the particles be likened rather to centres of force than to solid atoms?

14408. The varnish does not seem to trouble the general effect.

14409. Spaces which when *looked through* shewed only a dark translucent film, or with it a sprinkling of opaque parts within the investing chloride, when looked at by reflected light gave a surface apparently almost continuously metallic, so much effect was produced on the eye. Parts of the film which were translucent

still reflected a dull metallic lustre no doubt, but the eye is much affected by disseminated bright particles, and without the microscope or the glass they often present a continuous metallic appearance.

14410. Placed some aqueous solution of chlorine at the bottom of a glass and some of the silver leaf (14404) in the air above it. Slow action, and as the leaf was perpendicular, much slower action on the top than the bottom. The metal becomes more and more translucent but presents very little colour. It appears by transmitted light as a body always black, gradually disappearing, though where very thin and still able to reflect metallically, it presents dark and pale slate colour. I doubt the green tint as a true transmitted tint of silver.

14411. The well and completely made film of chloride silver is by transmitted light of a pale smoke colour—it does not depolarize polarized light. When not quite converted it has the purple tint of discoloured chloride of silver, and is in the coloured part no doubt in the same state—it then has more reflective force. Parts that have clear continuous metallic reflective power are translucent to transmitted light and must I think be transparent silver (14427).

14412. Pale Gold leaf on glass (14404) is by itself and viewed by transmitted light of a violet or blue colour. Being placed in a very weak atmosphere of chlorine, it became more and more transparent —a slate colour or brown came on—printed letters appeared, where probably the printing of the book had bruised the gold leaf—yet the metallic lustre by reflection strong—probably from particles of gold—more letters—after a time a greenish transparency came on to the plane parts of the leaf, and that not from the colour of the chlorine atmosphere, but as the chloride of gold formed is yellow, that with the original blue may probably have produced the effect. Parts became converted into chlo. silver before other parts had lost their gold reflexion; but the appearances were not good, for the film was much wrinkled every where and the chloride of gold crystallized in the different crack[s], wrinkles, etc. The effect nothing like so good as on solution of chlorine.

14413. Another set of the Pale gold on glass (   ) was put above solution of chlorine (14410). The gold became more and

more transparent—the bottom portions changing fastest. Beautiful violet transparency in parts here, and truly as I believe of the continuous gold (14426).

14414. Worked with Electricity—having the battery of 2 pr. plates (14363) and the gold solution as before (14376). Employed Tin as the Negative Electrode: the gold did not go down well upon it—scarcely at all. Added a third cell to the battery and then it was well precipitated i.e. covered the surface yellow.

14415. *Zinc* foil (14380). A piece made negative and gilt on both sides and edges—turned round so as to have every part gilt. A second piece gilt on one side only. Both laid on dilute Nitric acid—must not be too weak but sensibly acid to the taste. After a time, the action being slow, both were found to have gone so far as to be flexible, and after a longer time both shewed every undulation of the fluid. They seemed to go on nearly alike, so that the gold was no protection and must be porous. After a still longer period, the single film was removed, washed and placed on glass. It shewed every mark and imperfection of the surface of the rolled zinc—was of a slate or purple colour by transmitted light—very porous or broken up—it looked like a very irregular bad De la Rue's film, i.e. by phosphorus vapour. By reflected light it was golden, but dull or matted. It can scarcely be considered as a continuous film. No. 65 (14290).

14416. The zinc gilt all over (14415) gave two films of gold which fell apart—the upper one which floated was washed and laid on glass. In one place part of the under film was there, and so there was double thickness. It was a better piece than No. 65, but shewed marks of the zinc surface—it was spotted all over by the action of bubbles of air that had caught under the film. Between the spots were parts apparently continuous and transparent, transmitting a purple slate colour, and where the tint [? film] was double it was of the same colour though very dark. No. 66.

14417. Zinc foil alone placed floating on solution of chlorine. Zinc foil, gilt by Electy. on one side, placed on the solution with the zinc downwards—the zinc dissolved only slowly and the gold became dull and tarnished by vapour. Process not useful.

14418. Repeated certain results with Silver and Gold leaf on an

aqueous solution of chlorine. *Silver leaf* on a solution of chlorine gradually became a film of chloride as before ( ).

14419. *Pale Gold leaf* on solution of chlorine gradually became reduced to a very thin film of chloride of silver.

14420. *Deep Gold leaf*—on solution of chlorine. Dissolved more slowly, I think, than the pale gold—and finally left a very faint film of chloride of silver—No. 67. Six deep golds were put on a fresh solution of chlorine and after a little while taken off in succession, so that they form a progression. All are green by transmitted light, but the tint is less and less, the films being more and more transparent. By superposing them, the green tint becomes very rich, and by day light can see through the five thickest films at once. The tint not altered by superposition. Each plate has a high degree of metallic gold reflexion. By the lens they all seem transparent. They are numbered 68, 69, 70, 71, 72, 73—68 being the thickest.

14421. *Extra deep gold.* Seven pieces put on to solution of chlorine and left longer than before and with larger intervals of time. They all had a redder tint than the original leaf. All were well metallic by reflexion, but the last films were becoming specky and rotten. By transmitted light the original gold had a warm green tint, and the thinnest film and all the others had the same. The thinnest did not depolarize a polarized ray of light. In the thinnest the reflected ray becomes feeble and scarcely sensible in parts which, examined by the lens, still shew the colour of gold by transmission, and in these cases the necessity of a certain thickness for reflection appears very evident. The original leaf is No. 74 and the rest in order down to 82.

14422. The tube of red solution and particles of gold (14343 No. 2, 14355, 60), put into the concentrated voltaic light and looked at by the reflected light, was of a dead yellow tint and so obscure as to be opaque to the eye, so much light was sent back from the gold particles—but yet very much transmitted light passed thrgh., probably $\frac{4}{5}$ or more, and that gave on a screen beyond, either an intense light looking almost white, or if diffuzed, a fine violet colour. Is a beautiful experiment and would do well in a square glass cell. No. XXI.

14423. Went and talked to Mr Cuxon, 228 Tottenham Court Road, Gold beater—about the beating of Gold. First as to the alloys and colour—beginning at the deepest.

Extra deep	contains 7 gr.	copper and	6 grains	silver	to 1 oz. or 480	gr.	gold
Deep	contains 6 „	„	12 „	„	to 1 oz. or 480	„	gold
New	contains 6 „	„	21 „	„	to 1 oz. or 480	„	gold
Pale	contains 0 „	„	120 „	„	to 1 oz. or 480	„	gold

14424. The leaf finished as to beating and before they are cut, should average about $4\frac{5}{8}$ inches square. Supposing 2 oz. of such leaves were to be cut, an average result would be that the leaves would weigh 16 *dwt.* and the cutting 24 *dwt.* The leaves when cut are $3\frac{3}{8}$ square, and it is considered that 2000 of them would weigh 16 *dwt.*, or 384 grains. Now $3\frac{3}{8}$ squared gives each leaf as a surface of 11·39 square inches, and 2000 of these would give a surface or rather extension of 22,780 square inches, weighing 384 grains, or 0·01685 of a grain per square inch. I made it less than this, namely 0·0116 (14396), yet I assumed my leaves as smaller, namely $3\frac{1}{4}$ inch square. The Gold beater probably includes lost gold in his estimate of weight (14489).

14425. Pure gold cannot be beaten so thin as alloyed gold. When of a certain thinness, it ceases to spread in the tools or skins. Some was beaten by Mr Cuxon about 30 years ago for the ball, etc. of Bow church. He had to charge 5 or 6 times the price of the gold of a common leaf. It is now on the church summit—it is of course so much thicker. This non-extension of the pure gold is very curious.

14426. The Gold film on glass (14412) put into dilute and also into strong solution of ammonia to remove the chloride formed, which was done, and the gold appeared better; has the same violet slate hue as before. When dry the varnish still obscured it; washed with alcohol 2 or 3 times—removed much but not all the varnish, which has been altered by the chlorine—not much change; but the varnish is an interference.

14427. The silver films (14405) on glass treated by ammonia, strong and weak, and then by alcohol. Much brightened up but no further change. The varnish an impediment to observation.

14428. Though most of the Phosphorus films (   ) are blue or violet blue, yet No. 20, a thick film, is green and shews the green tint of beaten gold. Must trace this effect.

14429. Took the phosphorus film No. 19, which is very dark, and by pressure on it with a round glass surface endeavoured to imitate the pressure effect of beating. I could not succeed in producing any good compression of the place or any appearance of a green spot—but must try again.

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14430. At about 11 o'clk., pieces of deep gold leaf were laid on the following fluids in separate evaporating capsules in order to observe any solvent action; a piece of the same gold leaf placed at the same time on water for comparison.

- I. Strong Sulc. acid.
- II. Strong pure N. Acid.
- V. Caustic potassa.
- VI. Strong sol. ammonia. (14441)
- VIII. Water.
- VII. Sol. Bichromate potassa.
- IV. dilute mixture of N. and M. acids.

14431. III. Strong M. A.—considered pure—this dissolved the gold leaf freely, both on the surface and beneath, but apparently quicker in the former case. No chloride of silver is left, for that dissolves in the strong M.A. Must look closely at this difference of surface and under action, and see if an air voltaic circle is not formed. See also that when under the surface it is not the mere crumpling up that occupies the time.

14432. IX. 1 vol. the M.A. of III + 2 vols. water. Gold leaf either on the surface or beneath dissolved very freely, almost as in III. Gives a very clean reduced leaf; see No. 83.

14433. X. 1 vol. M.A. of III + 11 vols. water. Gold leaf in and on it at $11^h\ 45^m$; at $12^h\ 45^m$ there was considerable solution of the gold—the leaf was wet all over but floating at the surface crumpled up.

At 3 o'clk. IV Gold was dissolving—at 7 o'clk. it was thinner still.

14434. Nos. I, II, III, V, VI, VII, VIII apparently unchanged.
14435. Nos. IX and X, Gold leaf gone.
14436. No. IV—the gold leaf very thin but is there. I think the chloride of silver is not dissolved here and so retards the solvent process—perhaps this may offer some advantages.
14437. My red phosphorus gold fluid XX (14321, 42) on the cool part of the sand bath on Saturday. On Monday it was not dry, but films of Gold form at the edge—took off a specimen on glass marked No. 84; the film well golden by reflected light and violet by transmitted light, exactly like some of the phosphorus gold films. The fluid was acid as expected and still had colour, but more violet than before. There was a deposit at the bottom of the plate and fluid having the gold and purple character just like some of the common gilt ware obtained by heat. In this state it cannot contain phosphorus, and being insoluble and unchangeable by heat, the question is, is it in the same state as whilst apparently *dissolved* in the fluid.
14438. Some of the same solution placed on the seats to evaporate by common temperatures, and also some of De la Rue's red fluid of like nature placed there—the latter has nearly lost all soluble colouring matter and I have thrown it away. Exposure to air seems to cause gradual separation of the gold as a purple or violet coat or deposit.
14439. Some of my red fluid (14437) and Ether in a bottle shaken together—the ether separated colourless—shewing no signs of taking out the gold—neither did it destroy the colour or cause separation of the gold, in 48 hours.
14440. I had a glass containing old deep violet phosphorus fluid, XX (14321). It has been standing many days and the coloured body seems to have settled in part; also in what is apparently solution there is settling, for on moving it the part beneath moved up in striæ into the upper, having a deeper colour. The particles must be extraordinarily fine, for it is many days since they were left quiescent. Now poured off the coloured liquor, which of course became mixed together, and set it aside marked S at this date to settle. No. XXVI.

14441. Examined the deep gold leaves placed on different fluids ten days ago (14430)—comparing them in succession with that from water, VIII—after they had been transferred to water and placed on glass.

VIII is that from water—now made No. 86.

I. From *Strong Sulc. acid*—not dissolved or changed chemically but is puckered up and crumpled, so that though as thick and green as that from water, i.e. 86—it is more obscure. No. 87.

II. From over strong Nitric acid—no change, as VIII—is No. 88.

IV. From over dilute N.M.Acid—has been much eaten or dissolved away, but not during the last few days—more dissolved on one part than another—the chloride of silver remains undissolved and that invests and protects the rest of the gold. The gold thins beautifully—it has golden reflexion in every part but weak where it is thinnest, and it is also green in every part not passing into violet. No. 89.

V. From over caustic potassa—not dissolved and very little altered—a little paler than VIII, or No. 86, by reflected light and a little bluer in the green by transmitted light—No. 90.

VI. From Ammonia—just as the last—is No. 91.

VII. From Bichromate potassa. No solution, but some discoloration, as if chromate of silver formed. No. 92.

14442. *The red phos. gold fluids.* The tube of my fluid No. 1 (14343), or now XX, is not altered; there is no sign of settling.

14443. The tube No. 2 (14343, 422), now XXI, which had been boiled, has settled beautifully since the 13th instant (14422). Looked at sideways, the liquid is nearly colourless—looked at through the bottom, the settlement is almost opaque—but being inclined, striæ of colour quite transparent in appearance flow from the opaque part. The colour is a fine violet—though it looks transparent, it must consist of a deposit of solid particles excessively minute. With a little more motion, the middle of the deposit gradually lets light through, being at first of a deep blue violet which run in striæ and these by dilution become paler and ultimately of a rose violet—but clearly all consists of solid particles though thus coloured. When shaken up in the supernatent fluid,

all resumed the appearance it had ten days ago. This tube affords a very fine illustration of division and connects the phenomena with the division of the particles in the first tube. It was now left, having the same tint and appearance as that it had when first boiled.

14444. The red liquid in Glass S (14440), or No. XXVI, is now settling—just clearing at the top—the fluid as a whole has a very fine violet tint but with the hazy golden reflection when looked at from above (14452).

14445. My bottle of red fluid (14321, 42), now made XXVII, has a sediment in it at the bottom like XXI (14443), which by motion of the fluid stirs up, forming striæ of a violet blue at the first.

14446. The fluid left in a plate 10 days ago (14438), made XXII—is now dry—it has left a film deposit coloured violet, purple and even green in parts, and having golden reflexion just like the gold surface colouring on cheap pottery.

14447. The De la Rue fluid, made XXVIII, has a deep violet colour; a considerable portion of it boiled in a Florence flask and set aside, marked M or XXIII (14483).

14448. A piece of phosphorus into water—then added a little solution of Gold—the coloured deposit soon formed around it but on the whole it was not a good process, because of the adhesion of the violet gold to the phosphorus itself. On stirring with a rod, to which particles of phosphorus adhered, many of these particles floated—and fine thin films of gold etc. formed on the surface of the solution around them.

14449. Employed Phosphorus dissolved in sulrt. carbon as before (14321). Had two basins. Employed in one my acid gold solution (   ) and will call the results XXIV; and in the other, De la Rue's neutral gold solution, and will call it XXV; both seemed to do very well. Employed pretty strong solutions and obtained very deep coloured fluids. It seems to me that Proto mur. tin can not well test the solution for unreduced gold, for immediately after adding gold I could not find any by it. It seems to me also that the sol. chloride of gold dissolves the violet gold.

14450. The Ether bottle (14439)—the violet colour is still there. Whether it is diminished or not I cannot say.

14451. *Gold fluid observations.* The liquid XXIV of yesterday (14449), a little of which had been put into a tube also to rest. That in the glass seems to present signs of settling—that in the tube not sensibly so. The other liquid, XXV, that in the glass settling a little and also that in the tube.

14452. XXVI (14444) has settled since yesterday and now seems to be in a permanent condition, i.e. to shake up and resume its uniform state and then to settle again and so on.

14453. A portion of XXV (14449) was diluted 4 or 5 times and then boiled in a Florence flask—before boiling it was very golden by reflected light and very violet or crimson by transmitted light. After boiling it was more golden, but as crimson as ever I think. It was marked XXX and set aside to settle.

14454. A portion of XXIV (14449)—diluted about three times, which was not so yellow or reflective as the former and more transparent, was also boiled—it became more golden and was set aside to settle. Marked XXXI.

14455. The gold fluid XXIX is the after mixed results from the basins (14449) and may be considered as a mixture of XXIV and XXV. After being poured out of the dishes, the sinking films and residue, consisting of phosphorus and aggregated gold film, was washed by alcohol and then the phosphorus dissolved out by Sulphuret of Carbon—the gold film which was left was very small in quantity—and I believe just the same thing as the diffused gold in the fluids.

14456. I think the various process[es] employed all tend to forward the aggregation of the gold particles in one direction, i.e. from finer to coarser—the point is to know whether the finest or rose tint particles contain any phosphorus. Endeavour to obtain them in Sulphuret of carbon or dry upon Glass plate.

14457. The Red or ruby fluid XXIX (14455) in a tube with some sulphuret of carbon—shaken—the latter did not assume colour but it caused the adhesion of gold particles in films between the two fluids, and so lessened the colour of the supernatent aqueous fluid and changed it from ruby or rosy to violet. A little boiling lessened or weakened this colour—agitation took it all out of the watery part, and then the purple or violet films were

on the surface of the Sulrt. of carbon, wetted by it and between it and the water or glass—but the sulphuret of carbon itself was violet from particles diffused thrgh. it. There is no solution or chemical change of the gold by the sulphuret of carbon. Made XXXII. (14461, 14475).

14458. Ruby fluid XXIX (14455), a drop into a little Wedgewood basin, became purple at the edges and by moving about the whole changed from the ruby to pure violet tint. A portion spread over a white plate surface shewed the same change at different parts, producing a mottle; as if something adhering to the plate had caused the change—and so is not merely action of the air only. So the ruby fluid changes to violet very easily. In order to work this change out, put a little of the fluid XXIX into very clean Wedgewood's basins, and then by a glass rod added a drop or a touch of other fluids—allowed them to mix slowly so as to see the striæ of colour and observe the change. The alteration was always from the ruby tint to the violet, and as the tint came on the amount of colour diminished, i.e. a ruby liquid of a certain full intensity of colour became a violet liquid having much less intensity of colour. Probably the Gold, by aggregation of its particles, has been virtually removed.

14459. The following are some of the results with solutions of the bodies mentioned. *Common salt* turns the ruby to violet readily and well. *Sulphate of soda* causes the same change but not so quickly. *Caustic potassa* (   ): the same change—upon adding dilute Sulc. acid in excess, it did not cause a return, but the violet result remained. *Dilute Sulc. acid* added to the Ruby fluid makes it violet and seems to cause a separation, as if the colour inclined to flow in striæ arising from tendency to separate. *Dilute Nitric acid*—causes the same change from Ruby to Violet—the violet weakened so much as to make me consider whether the N. Acid had exercised any solvent power over the gold. Dilute proto sulphate of iron rendered the rose fluid violet. Proto Mur. tin acid—turned the ruby fluid to violet, and then quickly gave rise to separating flocculi of Cassius purple, and the liquid lost all colour; the association of the purple from the liquid with the oxide of tin here seems very simple. *Dilute Mur. acid* made the ruby fluid become violet—then adding a little proto chlo. tin

solution produced Cassius flocculi, but only slowly. Wants an attention to proportion, etc.

14460. The ruby fluid XXIX (14459[1])—weak solution of Gold added—changed from ruby to violet and the colour became so weak that one thinks of solution—but still the whole fluid remains feebly violet, and the yellow of the gold solution will cover some colour.

14461. *Alcohol*, *Ether*, *Camphine* and *Sulphuret of Carbon* (14457) applied in the same manner (14458) produced no change of colour.

14462. The boiled Ruby fluid XXX (14453), being of a ruby colour and as much so as XXIX just used (14459), was tried with common salt—the salt turned it violet, but the effect was not so strong though much more solution of salt was employed. I think a compound of phosphorus with gold could hardly have remained after the boiling.

14463. The various violet liquids produced (14459), excluding the tin experiments, were put together in one tube to settle and marked XXXIII.

14464. As far as I remember of all the experiments, the violet tints settle fastest—the rose tints slowest; XXXIII cleared at the top a little in an hour or two.

14465. A portion of XXIX was rendered violet by common salt—then a little of the solution of Phosphorus in Sulphuret of carbon was put on to it and agitated a little—but there was no tendency in the colour to return to the Ruby tint after an hour or two (14478).

14466. As to the effect of mere agitation—a little of XXIX was agitated in a bottle with air, and became violet. Another portion was agitated in a like bottle with mixed air and coal gas, and became violet. So it would seem not to be the air but the agitation which caused the change. So *filled* a smaller bottle with the rose fluid XXIX, and with a wooden agitator passing through a cork agitated it thoroughly; but it did not change to violet and also a larger quantity than before being agitated in a bottle with air did not become violet. There was probably some adhering impurity on the bottle which caused the change.

[1] ? Par. 14457.

14467. Chlorate of potassa is feeble in causing the change. Common salt is very powerful.
14468. The rest of XXIX was now made violet by common salt—marked XXXIV and set aside to settle. Even in a few hours it began to settle.

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14469. Red fluid XXIV (14449) in a bottle—to-day (from Yesterday) examined by reflected light—the top is clear across, about $\frac{1}{8}$ of an inch downwards, then it is below cloudy and at the bottom there is a layer of *brown* gold particles. By transmitted light it is transparent to the bottom, Ruby coloured at the very top and also downwards to the bottom—there is no difference in the character of the tint, only in its depth—as a cloud of particles it disappears—the only difference seems to be in their abundance. The fluid XXIV in the tube shews the same effect.
14470. Red fluid XXV left in a bottle since yesterday. By reflected light the sinking cloud has a lighter, i.e. a browner or brighter tint than that of XXIV. When a very bright light is thrown on to either of them, the red brown of the clouds rises up to yellow—the reflection of the Gold particles being more abundant.
14471. The Violet fluid XXVI (14440, 4, 52) in a glass has stood since yesterday; now examined it carefully. By reflected light it was dark and dingy. By transmitted light, violet transparent and with evident deposition on the bottom and sides. There was an internal gold reflexion from suspended particles in certain positions, as when the light was above and the eye on one side. Poured off the fluid; whether from the top or bottom it had a little violet colour of the same depth of tint in all parts. No loose sediment at the bottom, but the glass was coated with a violet tint on the sides and bottom—poured in distilled water and shook it rather strongly—then the whole looked like a glass of violet fluid, but on pouring out the water it came away colourless, and the glass remained violet as before. So we here obtain the violet substance as a washed and adhering film upon the glass.
14472. Tried to mix up this film by a stem of wood but the film

would not mingle with the water in contact with it; the wood swept it off and where it had removed enough it became stained (i.e. the wood) of a violet colour. Rubbed the coated surface with the end of a glass rod, which gathered up a good surface of metallic yellow bright gold—there is no doubt I think that this film is the same thing as gold exploded on glass by the Electric discharge. Such particles as could be shaken off into the washing water tended to give it a faint violet colour.

14473. This violet film, being washed, etc., was tested by various substances—it was not affected by strong pure nitric acid—nor by strong solution of caustic potassa—nor by pure hydro-chloric acid. Strong sulphuric acid displaced it from the glass but did not dissolve or affect it bodily. A little Nitro muriatic acid dissolved it, forming a feeble yellow solution, which being diluted—was precipitated by the addition of proto sulphate of iron—the precipitate and liquid having a greenish hue, as if the larger particles of gold were thus formed.

14474. The boiled ruby fluids XXX and XXXI (14453) derived from XXV and XXIV were just as the latter (14469, 70), except that the brown reflected colour of XXXI is somewhat lighter than that of XXIV. A very fine ruby tint is transmitted. Perhaps boiling will give a degree of permanency to the ruby state.

14475. The tube XXXII (14457, 61) of ruby fluid and Sulphuret of carbon is as last night:—but the sulphuret of carbon has *no* colour in it. It appears violet because the violet film is round it on the glass. When poured away to one side it is colourless.

14476. XXXIII.

14477. XXXIV. The portion of fluid XXIX remaining at the end of yesterday (14455, 68) was left in a glass after being rendered violet by salt. To-day the gold had all settled in adhering flocculi—leaving the liquid clear and colourless. Being shaken up, the fluid became a dirty obscure violet.

14478. The violet fluid and phosphorus (14465)—the violet body has not changed to ruby, but has settled as a violet film on the capsule. It was washed with alcohol—no change—with ether—no change—then dried. Then heated up to 400°, and the gold became more metallic and of a brown colour, as brown gold—or like a

red copper—a dull red heat brought it to the condition of brown and brown red gold.

14479. The ruby fluid XXVII (14321, 42, 5) has little or no trace of settling—filtered a portion of it through paper several times, passing it back into the same filter. It lost gold by the first filtration, the paper retained a stain and the fluid lost colour—a little colour was left even after the sixth filtration but then nearly all the gold had been retained as a violet deposit in the paper—small in appearance.

14480. This ruby fluid XXVII tried on a plate (14458) with gum and sugar—neither affected it—a little salt added caused the change to violet.

14481. The ruby fluid XXIV (14449, 69) was tried with gum and sugar in the same way and with the same negative results.

14482. Red fluid XLI. Some sol. of phosphorus in Sulrt. carbon was spread on the *sides* of a glass and after evaporation of the sulphuret a dilute solution of gold was poured in, so as to cover over the phosphorus that air might be excluded, and was left. It is now a very fine ruby solution and is to be left unaltered and undisturbed for a long time—covered over.

14483. My solution of gold G (14291) is now made XXXIX—and De la Rue's solution (   ) made XL.

14483[1]. De la Rue's ruby fluid boiled yesterday, XXIII (14447). When the flask was turned slowly up, the fluid was still violet—not having fully settled—and there were but little signs of deposit—nevertheless as the fluid moved back, it gathered up films from the surface of the glass; these were the insoluble violet, and by diffusion through the water they rendered it dull violet.

14484. XXXIII (14463). All settled and fluid colourless—the violet body was loose, not adhering to the glass; when shaken up, after much of the clear liquid poured away, it gave the rest a dull dirty violet appearance.

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14485. The various gold leaves (14423) were laid on to glass plates for experiment by means of water only—four or five of each specimen—as follows:

[1] 14483 is repeated in the MS.

I*a*, I*b*, I*c*, I*d*, I*e*. *Extra deep leaf*—was of a reddish yellow colour by reflexion—and of a warm green by transmitted light.

II*a*, II*b*, II*c*, II*d*, II*e*. *Deep leaf gold*—a fine yellow by reflexion, green by transmitted light but bluer than I.

III*a*, III*b*, III*c*, III*d*, III*e*. *New leaf gold.* Paler yellow by reflexion than the last—and by transmitted light of a bluer green.

IV*a*, IV*b*, IV*c*, IV*d*, IV*e*. *Green gold leaf.* Greenish yellow by reflexion—bluish purple by transmitted light.

V*a*, V*b*, V*c*, V*d*, V*e*. *White gold leaf.* White by reflexion—so thick as to be opaque to transmitted light.

VI*a*, VI*b*, VI*c*, VI*d*, VI*e*. *Silver leaf.* White by reflexion—so thick as to be opaque.

14486. The Gold leaf laid on glass by water and dried does not adhere to it very well. If dipped into water, taken out and dipped in again, it strips off and is left floating on the surface of the water; unless the glass be introduced with the gold on the under surface, and then it adheres and can be worked with.

14486[1]. Extra deep gold leaf (14485) I, heated in a spirit lamp flame on the glass side—the gold leaf did not change in appearance by reflected or transmitted light, but when the flame was urged by a blow pipe up to softening of the glass—then there was a change by transmitted light—the green faded away into pale gray and at last the gold seemed gone, yet by reflexion there was much brilliancy at the place, though not so much as w[h]ere unchanged by heat. It seems as if the particles had been swept away by the current: yet what is left is not green. Must repeat this in close tube or between two glasses. In the altered places the gold adheres strongly to the glass. No. 93.

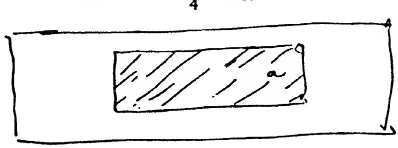
14487. White Gold leaf V (14485). Exhibited a like general effect by a like application of heat. No. 94.

14488. Gold film by phosphorus, No. 10 (14290), a strip about this size on glass*[2]—pale yellow by reflected light—slate colour by transmitted light; being *heated*, and more about *a* than elsewhere—the character did not change at first, but on applying the blow pipe to the glass side, the part about *a* became by reflexion an orange yellow, apparently of more body and throwing

[1] 14486 is repeated in the MS.

[2] Reduced to $\frac{3}{4}$ scale.

* [14488]



much light back to the eye in position wher[e] the unchanged parts returned but little light. The lustre was metallic. The effect was the same in kind whether the gold was looked at through the glass or not. No part of the gold seemed removed or carried away. By transmitted light the part was as deep in colour as before, but more violet in colour. The gold was not fixed to the glass in any part; not burnt in as the former. Yet I think it was as hot. Examine by Microscope. This and other films change tint by inclination. No. 95 (14494).

Difference of reflected and transmitted light greater than before.

14489. *Thickness of Gold Leaf.* Mr De la Rue has sent me his results as to the weight and thickness of Gold leaf: see the table [1]. The thickness and weight is very near that of the gold beater (14424) and more than my first (14396). As to the second, I have put 20 leaves of pale gold (very purple by transmitted light) into alcohol to collect and weigh them (14492).

14490. If the reflexion of metals be internal, think of the quantity which can enter into and return out of silver. Perhaps $\frac{9}{10}$ths.

14491. For a colourless thin film for comparison, refer to the film of Prussian blue round a crystal of red ferro pruss. potassa and a solution of iron. Perhaps procure such an one by pouring one strong solution on another, and send rays through it. Moisten Glass plate well in red ferro pruss. potassa and then dip it steadily and quickly into a solution of iron.

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14492. The 20 leaves of pale gold weigh 3·9 grains (14489), i.e. 0·195 of a grain each. This is more than before (14396) and close upon the estimate of the Gold beater and of De la Rue.

14493. The red fluids, etc. looked at (see 28 Feby.).

XX: fluid in tube unboiled—not settling.

XXI: same boiled in tube, rosy, etc., settles beautifully—deposit fine rose red, dense: transparent by transmitted light—stirs up into ruby striæ in the water—fine case of *solid* particles, ruby coloured, settling, yet diffusing again.

[1] On p. 54. The table as received from Mr De la Rue is bound into the Diary.

Weight and thickness of Gold leaf, De la Rue (14489)

Deep Gold

Specific Gravity 18·35
5 books containing 125 leaves fused with borax
weighed 1·5728 gramme – 1 leaf averaged therefore 0·01258 gramme

	Progressive No.	Length inches	Breadth inches	Area square inches	Weight gramme	Weight per square inch gramme	Weight per cubic inch 1 cubic inch of water at 62° F. assumed to weigh 16·359 grammes } *252·03 grains*[1]	Thickness	
book No. 1	I				0·0125		Grammes		
	II				0·0128		300·18765		
	III				0·0120		*or 4624·78 grains*[1]	inch	1
	IV	3·3750 ×	3·3575 =	11·3316	0·0121	0·00106781		0·000003557	281124 of an inch
	V	3·3600 ×	3·3625 =	11·2970	0·0121	0·00107108		0·000003568	280266
	VI	3·3625 ×	3·3675 =	11·3232	0·0125	0·00110393		0·000003677	271926
	VII	3·3550 ×	3·3525 =	11·2476	0·0125	0·00111134		0·000003702	270113
	VIII	3·3550 ×	3·3625 =	11·2812	0·0128	0·00113463		0·000003780	264569
book No. 2	IX	3·3725 ×	3·3650 =	11·3485	0·0120	0·00105741		0·000003523	283889
	X	3·3725 ×	3·3700 =	11·3653	0·0116	0·00102066		0·000003400	294111
		3·3650	3·3625	11·3135	0·01229	0·00108141		0·000003601	277999
					or 0·18930f gr.[1]	*or 0·01666 of grain*[1]			

Lemon Gold

Specific gravity = 16·77
5 books containing 125 leaves fused with borax
weighed 1·5078 gramme – 1 leaf averaged therefore 0·012006 gramme

	Length	Breadth	Area	Weight	Weight per square inch	Weight per cubic inch	Thickness inch	Thickness
						274·34043		1
XI	3·3000 ×	3·2850 =	10·8405	0·0125	0·00115308		0·000004203	237919 of an inch
XII	3·2975 ×	3·3000 =	10·8817	0·0122	0·00112114		0·000004087	244697
	3·29875	3·2925	10·8611	0·01235	0·00113711		0·000004145	241308

[1] Additions in Faraday's handwriting.

List of objects, continued from page 2946[1].

No. 96. Phosphorus Gold film, 17, heated by blow pipe (14494, 566, 626, 74, 6).

97. Extra deep gold leaf—heated between glass (14496, 624) and pressed (14531, 14673).

98. White Gold leaf heated between glass (14487, 624).

99. Silver leaf heated between glass (14498).

100, 101, 102 } (14531)—Phos. Gold film—weak solution.

103 —14567—are as No. 180.
104 —14567—Do.
103–107 } —(14532)—Phos. gold film—stronger solution. Good—thin and regular.

108 —14671.
109 —14567.
108–111 } (14533)—Phos. gold film—strong solution, long time.
111 No. 111 (14536)—salt and N.A.; no action on the film.

115 —14647.
112–120 } —(14534)—Phos. gold film—strong solution—strong phosphorus—in dark.
118 14671.

121 to 129. Parts of the same leaf of deep gold—121 not altered at all, 122 to 129 (14620) thinned by sol. cyanide of potassium (14669) more or less (14499).

130 to 139. Parts of the same leaf of silver—130 not altered but the rest thinned by sol. cyanide of potassium (14500, 14669).

140 to 148. Parts of the same leaf of copper: 140 not altered—the rest thinned by surface contact with solution cyanide potassm. 141 on the cyanide and off again directly (14505, 623, 69).

149–155. Copper films *immersed* in the old solution of cyanide (14507).

156–164. Dutch metal in and on the sol. cyanide (14510).

165. Silver film off Nitric acid good (14511). Also No. 166 (14623).

167. Silver from under cyanide potassm. (14519).

168. Silver from under cyanide potassm. and salt (14523).

169–173. Silvers on solution of chlorine. (14524, 14669).

List continued at page 3004[2].

[1] See p. 18.
[2] See p. 67.

XXIII. Settled–a violet deposit, moving in striæ–is in all physical characters like XXI, except being violet instead of ruby–good case of fine solid particles.

XXIV. Settled, so that at the top almost colourless for about $\frac{1}{8}$ of inch down–and XXV the same–the tubes XXIV and XXV the same–fine case of deposition of particles giving colour.

XXVII: a trace of settling at the top.

XXVIII: no settling.

XXX: Settling–cloud below–and that moveable, being carried up on the warmer side of the flask.

XXXI: Settling–the cloud lifting, as in XXX.

XXXIII: clear–settled–shaken up becomes violet.

XXXIV: as XXXIII (14594).

XLI: beautiful–not changed–no settling–ruby colour. (14544).

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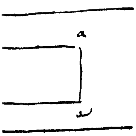

14494. Heat applied (14486, 8) to a fine equal piece of De la Rue film, being No. 17, by the alcohol and blow pipe flame, as before. All parts were made pretty hot but the heat was most intense at *a*, *a*, and least at *b*. At *a*, *a*, the crown glass supporting the gold was softened and bent. By transmitted light the tint at *b* was unchanged and I think even when hot there was no change, being a violet slate colour. At *a*, *a*, it was changed to a warmer violet, an intermediate greenish tint coming between–by inclination of the film, all the tints deepened in colour. Much more light was transmitted thrgh. *a*, *a*, than thrgh. *b*, as could be easily seen by reading a book thrgh. the parts. By reflected light *b* or the original part was more golden than *a*, *a*. About *a*, *a*, the gold seemed reduced to the brown state, being probably run up into globules–the parts between are intermediate–by inclination several tints appear and there is a mother of pearl effect–even a white appears in some positions and the play of light seems to shew the effects are due to the interference of reflected rays. There are also differences as the gold is regarded on the uncovered face or on the face against the glass.

14495. A soft wooden point carried from *b* to *a* easily removed the gold until nearly at *a*, and there also if pressed on forcibly.

So no fusion into the glass. My microscope only shewed a uniform film. Must see if De la Rue's resolves the film at *a*, *a*, into fuzed particles. No. 96.

14496. Extra deep gold (14485) covered by a thinner glass and then heated by the blow pipe as before (14486) but without any chance of blowing away the particles. Same change as before but carried further. The reflexion at the heated part was much less. What there was was golden but there was also a little play of light by inclination, like that on the last case (14494). *Transmitted* light. The green colour was gone and only a mottled grey left. The gold seemed gone, but must be there, probably in a fuzed retracted state in particles, according to the cohesive principle well described by Plateau in speaking of his wire or liquid cylinder. The particles, etc. opaque by transmitted light are those which *reflect* golden yellow light. The general tint of the ground where not opaque is by transmitted light a pale gray. It is evidently not clear, for in the microscope the difference between it and the clear places due to breaks in the gold leaf originally, are very different indeed. If it be due to an exceedingly thin film of gold, then it is too thin to be either green, blue or violet or ruby. Must look at it in the Microscope. No. 97.

14497. White gold leaf (14485) heated as just described between glass. Exhibited the same change as before (14487). So the alteration not due to the dispersion of the metal, but probably to its running into masses by the heat. No. 98.

14498. Silver leaf (14485) heated in like manner between glass. The same kind of change and due no doubt to the same cause. No. 99.

14499. A leaf of deep gold (14485) divided into nine parts. One laid on glass by water and marked No. 121 as a standard; the other[s] laid on the surface of a solution of cyanide of potassium for a shorter or longer time, then transferred to water, washed and then transferred to glass. No. 122 was on the cyanide about half a minute—the other[s] for increasing periods. The action of the cyanide was as before, to dissolve and thin the gold—the upper surface of the metal on it soon dulled a little, as if action there from vapour or the fluid passing through it, and there is reason for that action (   ). The reflexion from all is golden and

unchanged in character except that the more reduced the film the less reflexion. All are mottled and irregular—the very thinnest have the same green by transmitted light as the thickest, but reduced in character and in the thinnest becoming grey. Still, there seems to be a film of metal even when the green passes into grey. Some, as 124, shew an appearance of red particles like a worm track near a [illegible], but whether the gold is really red there or the effect is simply that of contrast of a cleared track with the green neighbourhood, I cannot say yet (14743).

14500. A leaf of silver cut up and dealt with as the gold just referred to (14499), Cyanide of potassium being the solvent; the pieces are numbered in order from 130 to 139, the lower numbers being the thicker metal. By reflection all are silvery. By transmission it seems that silver in dissolving does not leave a smooth surface and I doubt if what appear as films are any thing else than frame work of ramifying particles. Must examine them by a microscope and also make them Electrodes (14743[1]).

14501. The silver requires more time than the gold to dissolve on this solution, but then the leaf is much thicker. Ultimately, when light permeates it, it is more granular than the gold, so that the films are like coralline ramifications.

14502. Both gold and silver dissolve away far more rapidly when lying on the surface with the upper metallic surface dry than when the leaf is dipped in the solution. At 9^h 45′ P.M., some gold leaf was put on to the surface of a portion of the fluid in a glass, and some gold leaf was also placed beneath the surface; at 9^h 57 the surface gold, which was doubled in several places, had dissolved, leaving a mere ghost behind, all having been quiescent in the meantime. At 10 o'clk. the gold beneath seemed still untouched. Left all till next morning at 9 o'clk., i.e. for about 11 hours, and still, though some gold was dissolved, I think a third must have still remained.

14503. Some Silver leaf dealt with in the same way, at the same time—the surface silver dissolved away in a few minutes. That beneath the surface was about half dissolved by the next morning, and what was left was considerable though brittle, and by stirring

[1] ? 14744.

soon broke up, as if the mass tended to be granular or dissected out (14504).

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14504. There were to-day some little pieces of silver still left at the bottom of the glass, or pieces of what appeared to be silver (14503).

14505. *Copper leaf* on cyanide. A leaf of copper cut into pieces and laid on to the same solution of cyanide of potassium which has been used for the gold and silver (14499, 500), and which must therefore contain gold and silver. The copper is well acted on, and usually becomes rotten and breaks up. The resulting specimens are numbered 140–148. Many of them appear full of holes—some of them, as 143, 144, 145–148, give appearances as of a continuous film of transparent metal. But I am in doubt whether these may not be films of subcyanide of the copper, or perhaps precipitated films of silver. Must investigate these points.

14506. As the copper appeared to receive a peculiar tint on the side touching the solution of cyanide—and as it appeared to dissolve in a different manner when *both* surfaces were under the solution; so some of the copper leaf was quite immersed—in about 20′ it was dissolved, leaving a floating black film. So much slower than when the upper surface touches air.

14507. Portions of copper leaf were therefore immersed in the same solution of cyanide and left for a longer or shorter time. No. 149 was dipped for an instant only, to affect the surface—then taken out, washed and laid on the glass. Nos. 150, 151, 152, 153 were in for a very much longer time, and some of them, as 151, 152, 153, give indications of a transparent film, but it is green as of gold—or it may be subcyanide. If gold it will be interesting. Nos. 154 and 155 were very long underneath this old cyanide; they are now white like silver, and the only sign of transparency is a spot on 155. Otherwise they appear as thick precipitated films of silver (14525).

14508. Some copper leaf was put into sol. of cyanide silver in cyanide and left there for $1^h\ 40^m$. There was then plenty of metal left which was white, looking like silver—washed—dissolved in

Nitric acid—evaporated to dryness; left a film not cupreous and yet not nitrate of silver merely, for chlo. sodium did not give its proper precipitate. Is it a sub cyanide?

14509. Some copper leaf in a like solution of gold for the same time gave a yellow film, which being washed, then dissolved in N.M.Acid, gave no trace of gold to proto chloride of tin: must examine further (14526).

14510. *Dutch metal* and the old cyanide solution (14500, 5) Nos. 156 to 164. The metal very full of holes. Nevertheless in some of the specimens, as 159, there are signs of transparent films—but whether of Gold, silver or insoluble cyanide of copper I cannot yet say. No. 163 was altogether immersed. No. 164 had one corner immersed, dipping down in the solution—there there is an altered surface and a transparent film, but it is green and may be golden in character.

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14511. Nitric acid as a solvent more or less diluted—1 *vol. pure N.A.* + 2 *vols. water. Silver leaf on* its surface and *in* its mass—both dissolve in a minute or two—and the one immersed most quickly and with the evolution of a little gas. The one *on* seems to promise pretty well—a thin film remains even after six minutes. The browned edge of the leaf (sulphuret of silver) did not dissolve but was left as a brown film. The silver film from the surface of this acid after six minutes is No. 165 and a good film. Seems to be an irregular but continuous transparent leaf, also No. 166.

14512. 1 *Vol. N.A.* + 5 *vols. water. Silver leaf on* and *in* the fluid. Both were long in dissolving—the leaf immersed went first and in four hours was nearly all gone. That on the surface had in the same time dissolved in part—had become wet on both surfaces and sunk—it gave no appearance of gas evolution. In 2 hours it had gone much, but half of the piece in a thin state yet remained.

14513. 1 *Vol. N.A.* + 11 *vols. water. Silver leaf* on and in. Not much affected after $9\frac{1}{2}$ hours—the upper one dull, the immersed one bright. 24 hours more (Eveng. of Tuesday[1]), no sensible

[1] Pencilled note.

difference—24 hours more, both silvers a little thinned—but no useful effect.

14514. 1 *Vol. N.A.* + 23 *vols. water. Silver leaf in* and *out.* Not sensibly affected after $9\frac{1}{2}$ hours—24 hours more, no sensible difference—24 hours more, no sensible difference.

14515. 1 *Vol. N.A.* + 2 *vols. water. Copper leaf on* and *in.* Both dissolve at once and about as the silver (14511), the one underneath quickest. After 5 minutes thin films were left, apparently not metallic; at the end of 6 hours they remained. They did not dissolve in N.Acid by heat but did dissolve in N.M.Acid. They were black when crumpled up and looked like sulphuret. The copper leaf is tarnished by oxidation or sulphur, besides which it is not very clean from the book; perhaps a little dust on it. The acid here used was that of (14511) and would contain some silver, so made a fresh mixture of 1 Vol. N.A. + 2 vols. water and put some of the same leaf of copper *into* and *on* it. That in dissolved at once, with a little evolution of gas and leaving very little film. That on the surface dissolved quickly, leaving a film, some of which remained after 32 minutes and even after 3 hours; and a trace remained an hour later.

14516. 1 *vol. N.A.* + 16 *Water* with a little Mur. acid added. *Copper leaf* on and in. That on the surface after 24 minutes left a mere film, whilst that beneath was not as yet sensibly affected. In $2\frac{1}{2}$ hours more, that immersed was going and very thin—but 3 hours after that a portion of it still remained. No use in N.A. as a solvent for the copper.

14517. Fresh cyanide of potassium solution, consisting of 1 vol. saturated solution and 5 vols. of water, employed—it has been filtered and is of an orange colour, but contains none of the common metals. When employed, that used for Gold is to be kept for gold, and that employed for silver kept for silver for the future.

14518. Cyanide solvent (14517) and *gold leaf* (deep) both *on* and *in* the liquid. That on the surface dissolved at once, leaving no residual film. Good. That beneath not sensibly affected at first, but after 35 minutes was going and in 80 minutes was gone entirely. A good solvent for gold—perhaps may be employed weaker.

14519. Cyanide solvent (14517). *Silver leaf on* and *under*. That on the surface much affected in 9 minutes, and in 3 minutes more was gone, leaving no film. That beneath was apparently unchanged in these 12 minutes. At the end of 3 hours it was apparently unchanged. In 3 hours more it was affected and became rotten, and being washed in pure water, was laid upon glass. No. 167.

14520. Cyanide solvent (14517). *Copper leaf* on and under. In two minutes, that at the surface thinned, broken up and almost gone–*no film formed*–goes entirely. That also beneath appears red, cupreous and undiscoloured. After 40 minutes this leaf was going, evolving a little gas, and in 2 hours more it was all gone.

14521. So the cyanide must be free from other metals and then it is good and gives good results.

14522. Some of the cyanide (14517) had caustic potassa added to it, and then gold leaf laid *on* and *in* it. That beneath now went quicker than that on the surface and hanging in the fluid; in 5 minutes its upper half was gone, as if the parts were in different fluids and formed a voltaic circuit. That on the surface remained whole and opaque a while longer, but after a time went irregularly, as at one edge and in a patch in the middle. The potash does no good but harm.

14523. Some of the cyanide (14517) with a little chloride of sodium added. *Silver leaf on* and *in* it. In 7 minutes that on the surface much thinned–in 10 minutes more still some film, but in 5 minutes more that gone and no film left. As to the immersed leaf, in 3 hours it was not sensibly affected. In 3 hours more it gave indications of action, but still was a thickish leaf though brittle; a part taken out, washed and placed on glass, though in a crumpled state. No. 168.

14524. A weak *solution of Chlorine*. Silver leaf laid on to it left for a time–then removed and laid on water–after a little solution of ammonia poured on the water to float the silver and remove the chloride formed–then the film removed to pure water and finally placed on Glass. In this way very good films were obtained; see Nos. 169–173. The action seems quick at first and then retarded by the coat of chloride formed. This No. 170 was 5 minutes on the chlorine. No. 171 was 8 minutes on it. No. 172 was 11 minutes on. No. 173 was a silver on glass (14485) vid,

put into the solution of chlorine with the metal downwards and hence its crumpled state.

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14525. The old cyanide (14507, 8, 9) had as much as a whole copper leaf added to it, all of which gradually dissolved, leaving at first films, but ultimately these also dissolved.

14526. Some copper leaf in the cyanide of gold (14509) for 24 hours—a film left looking like gold but when washed and acted on by N.A. much of it dissolved—the dark portion left dissolved in N.M.Acid and the solution was yellow—being evaporated and heated in the spirit lamp flame it left a spot with clear traces of gold, metallic, and on the glass side the spot was purple as of gold.

14527. Thin sheet lead put *on* and *under* a solution of cyanide (14517) at 5 o'clk. In 24 hours, no sensible difference.

14528. Thin sheet tin put on and under a solution of cyanide (14517) at 5 o'clk. In 24 hours, no sensible difference.

14529. As to the solubility to be expected of lead and tin, it is not as of silver, copper and gold. A drop of nitrate of lead, being precipitated by a little cyanide of potassium, required much more cyanide to dissolve the precipitate. A drop of chloride of tin treated in the same way indicated a greater solubility of the cyanide of tin.

14529*b*. *Pressure*. Took No. 95, which has been ignited at one end so as to change and fix the gold film. Place[d] a part at the heated end with the glass side on a little paper cushion, and then put on the gold above the convex surface of a rock crystal plano convex lens and pressed it by hand steadily, rocking it a little. This pressure converted the violet or dark tint of the place to a beautiful green—far more beautiful than any I have seen in gold leaf beaten—the effect was perfect. Has the pressure converted the layer of atoms into a continuous layer by expansion and welding, and is that all the difference? I rather think it is. No. 95.

14530. Gave a like pressure on the lesser heated and altered part of the phosphorus film, and obtained the like green there, though not quite so beautiful. So it appears that these different layers

are all gold, and owe their different appearances not to composition but to physical differences.

14531. No. 97. Deep gold heated until it had lost it[s] body in appearance—taken and pressure applied in like manner. The place appeared burnished—reflected as gold leaf—transmitted less and the colour transmitted was an emerald green. Even the ordinary act of burnishing with the convex surface of the rock crystal made the same change. Excellent.

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14531[1]. Made some Phos. gold films ( ). Drop[ped] 6 drops of our shelf solution of gold on to the flat glass plate by means of a glass rod, spread it over a surface of about 3 inches square inverted it and when the fluid level, placed it over the phosphorus dish in light. There was reduction, but the film seemed mobile and in curls, and I think the solution is too weak. It was left over the phosphorus 50 minutes, then transferred to the water basin: it ran up together as if too feeble. Portions taken off on to glass plates are numbered 100, 101, 102. By reflexion golden, by transmission slate colour.

14532. Six drops of De la Rue's stronger solution of gold ( ) dealt with in like manner, except that the phosphorus basin had more phosphorus than the former. After 15 minutes it was transferred to the water basin and gave very good films. Numbered 103, 104, 105, 106, 107. These films were also made in the light and it seemed to me as if the mere act of transferring them to the water stiffened them. These films are thin but regular. Light reflected from the metal surface is a little golden, and if the film may be taken for continuous, shews beautiful[ly] the necessity of a certain thickness for reflection. On the glass surface of the metal, the metallic reflexion is almost entirely deficient. By transmitted light the colour is violet green, especially when 4 or 5 plates are employed at once.

14533. De la Rue gold again, in light and as before (14532). The effect of reduction appears to go on much quicker at first than afterwards. It was left over the phosphorus $1\frac{1}{2}$ hours—then removed, washed, etc., and gave Nos. 108, 109, 110, 111. These

[1] 14531 is repeated in the MS.

films are much thicker than the former. Have a high gold reflexion. Colour by transmission, violet slate, passing into fine green—a green as beautiful as that of gold leaf, though not compressed: in 108, 109 fine clouds and striæ of green pass gradually into the greyish violet.

14534. De la Rue gold again, as before (14532, 3) except that the phosphorus was increased and the plate and basin were put into an obscure and nearly dark place; in 15 minutes the appearance was very golden—in 30 minutes it was taken off and the film transferred to glass, Nos. 112 to 120. Now the whole of this film was marbled, i.e. striæ and veins were there, formed at the first whilst the film had motion; and though soon fixed, yet thickening differently in gold so as to give variations. By reflexion the whole is golden. By transmission the thinnest places are slate violet, the thickest are green—and some fine green effects are seen in 116, 117, and also sudden spots of it in 118, 119. That which is green by transmitted light is also the part having most reflection and looking by reflexion as the most solid, for the striæ and veins are visible by reflected light. They are the parts which seem most continuous. Grey overlapping grey does not make a green—and it does not appear that the green is due to a mere obstruction of light or accumulation of tint; but to the joint action of the mass of particles in one film, whether considered as forming a porous or a continuous mass.

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14535. At the end of yesterday's operations (14534) the phosphorus basins were put into the washing water and all left during the night. This morning the fluid was of a fine ruby colour with a film of gold on the surface. Some of these films were taken off: they were golden tint but brassy—much broken up and rotten—very thin—like the others in character but not so good. The liquid was cleared of the floating films and then poured of[f] from the phosphorus pieces below—there was no red or golden sediment and the colour was very fine: put into a bottle and marked XLII.

14536. No. 111, being a fine green film, had a drop of salt and water applied at the place marked S, left a few moments, removed

by paper, the place washed twice and then dried; no change of colour was produced (   ). Another spot marked N was treated with dilute Nitric acid in the same manner, but there was no change in the colour there.

14537. Some of the metals on glass heated as before (14494).

No. 174. Extra deep gold—highly heated (14496)—effect as before.
175. Deep gold—heated—same result.
176. New leaf gold (14485)—heated—same result.
177. Green gold leaf (14485).
178. White gold leaf (14485)—effect like the former (14497)—broke in heating.
179. Silver leaf heated (14498)—effect as before.
180. Is film 106 which has been heated (14494).
181. Is film 110 Do.
182. Is film 112 Do.
183. Is one of the films 113 heated and the other left for compasn.

14538. No. 178. Is now by reflection very like mother of pearl, and one could imagine had the same surface condition. In some small parts the reflective power is almost gone. It adheres very powerfully to the glass. By transmission, there is a good deal of yellow and orange colour, as if the silver had combined with the glass. Must try pressure here and burnishing.

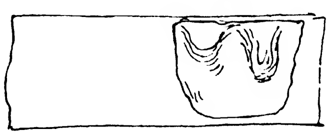
14539. No. 179. Silver heated. The retraction of the silver here is very interesting as shewing how Plateau's capillary action breaks up and divides a plate of irregular thickness when softened or rendered fluid, and this is probably the key to the rearrangement of the metals in all these heated cases.

14540. No. 180 film heated—has changed in appearance as before (14494) but is not fixed to the glass, and the transmitted colours are still purple, slate and green.

14541*. No. 181. When it was 110, it was fine green in various places, and many green veins or striæ. This green was changed into the common tint, a violet or greenish violet. There was the surface play of light by reflection, as if the surface were not polished, but sandy or rough. Notwithstanding the heat applied, the gold *did not* adhere to the glass.

14542. No. 182. Whilst it was 112 was green at the upper edge

* [14541]



List of objects continued from page 2990, 2946[1].

174–179. Gold and silver leaf on glass—heated (14537, 623, 73).
180–183. Phos. gold films—heated (14537–43, 14563, 4, 5, 626, 74, 6).
184–193. Gold and silver leaf on glass—heated, 14624, 73.
194–196. Gold films heated, 14567–9, [6]73, 4, 6.
180. Fine film heated, 14563, 5, 14532, 626, 74—becomes green by pressure.

194. Gold film heated, burnishes green, 14567, 674, 6.
195. Do. . . . . Do. . . . 14568, 674, 6.
196. Do. . . . . Do. . . . 14567, 9, 674, 6.

197. Gold film, phos., 14570, 7, 87, 625.
198. Do. . . . .
199. Do. . . . . heated, 14578, 87, 97, 626.
200. Do. . . . . heated, 14578, 87, 97, 674, 6.

201. Do. . . . . 14571, 7, 88.
202. Do. . . . . heated, 14578, 88, 676.
203. Do. . . . . heated, 14578, 88, 97, 674.
204. Do. . . . .

205. Do. . . . . 14572, 7.
206. Do. . . . . 14572, 7, 89.
207. Do. . . . .
208. Do. . . . . heated, 14578, 89, 674, 6.
209. Do. . . . . heated, 14578, 89.
210. Do. . . . .

211. Do. . . . . 14573, 7, . . . heated, 14578, 90, 676.
212. Do. . . . . heated, 14578, 90, 9.
213. Do. . . . .
214. Do. 14573, 7, 90
215. Do. . . . .
216. Do. . . . .
217. Do. . . . . heated, 14578, 90, 626.
218. Deposit from XXXI, ruby fluid, on glass, 14579, 600, 36.
219. Do. . . . . 14579, heated, 14600, 36.
220. Do. . . . . 14579, 600, 3, 27, 36.
221. Do. . . . . 14579, heated, 14600.
222. Do. . . . XXIII, violet fluid . . . 14579, 601, 36.
223. Do. . . . XXIII . . . . 14579, heated, 601, 36.
224. Deposit from XLV, ruby by salt, on glass, 14591, 605, 37.
225. Do. . . . . 14591, 605.
226. Do. . . . . 14591, 605.
227. Do. . . . . heated 14591, 605, 37.
228. Do. . . . finer state . . . . . 14592, 606, 37.
229. Deposit from XXXI, on glass, 14595, 604, 36.
230. Do. . . . . 14595, 604.
231. Do. . . . . 14595, heated, 604.
232. is Gold film No. 105—heated, 14598, greens well, 14625, 74, 6.
233. Do. . . . . 107—heated, 14598, Do. reheated 14603, 25, 74, 6.
234 to 243. Voltaic deflagrations, Gold terminals, 14664, 75.
235, 237, 239. Voltaic deflagrations, heated 14885.

See on to page 3042[2].

[1] See p. 55 and p. 17.

[2] See p. 99.

of the gold towds. the end; now that green is gone. The heat was high and applied on both sides of the glass, i.e. upon the face of gold as well as the face of glass. The gold adheres more than in the last case, but one passage of a wooden point takes it off. There is not the same adhesion as with leaf gold. Perhaps the colcothar acts as a flux.

14543. No. 183. Shews the difference of heated and non-heated film well.

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14544. Looked again at the various liquids and sediments (14493). See 5th March.

XX. Still not settling.

XXI. Ruby sediment as before.

XXIII. Violet sediment as before.

XXIV and XXV tubes—as before—deposition.

14545. XXIV. Clear and colourless at the top—ruby below—sediment at the bottom—poured off the top and part of the lower thicknd. fluid with fine sediment with it and it now makes the XXIV. The deposit looked through was ruby coloured—that left at the bottom had distilled water added to [it], but the sediment adhered chiefly to the glass, not shaking up with the water as XXI sediment does. When stirred up by a splinter of wood, it easily rubbed off the glass, and then gave a dull effect of diffused particles, but nothing like so good as the next, i.e. XXV. This sediment was dismissed.

14546. XXV. Was as XXIV in general appearance and state—colourless at the top—ruby below—and much deposit at the bottom. Poured off the supernatent fluid and made it XXV L, i.e. liquid.

14547. The deposit with liquid about it is XXV D. The deposit looks like brown gold by reflected light, but by transmitted light is of the purest violet where very deep and ruby where less so. When a drop or two of this mud was dropped into distilled water—reflected light shewed the striæ as brown gold opaquish streaks, but by transmitted light the striæ were of a fine violet or ruby colour, according to the depth of tint; and this effect occurred when more of the deposit was added, so that at last the sediment

and distilled water looked like a very rich sample of XXI. The sediment in XXV did not stick to the bottom or side of the bottle. We shall see, now that the mother liquor is removed and distilled water added, and it made XXV D, [if] there will be any change or adhesion. It is at present a beautiful preparation.

14548. XXVII and XXVIII, just as before (14493). Except that I think XXVIII is more violet than it was, as if changing by a slow action of aggregation or a slow salt action.

14549. XXX. Very like XXV. Same partial clearing and sediment—same looseness of the sediment—same power of the sediment. The supernatant ruby liquid poured off and made XXX B. Put distilled water into the flask on to the sediment and obtained just such a preparation as XXI. Still, call it XXX A.

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14550. This morning, XXX A had the same characters and appearance as yesterday, except that I think the solid particles are aggregating and becoming more gravitating and massy.

XXXI. Very beautiful liquid and sediment—the latter ruby.

XXXIII. Deposit violet, but coarse and aggregated.

XXXIV. The deposited particles cling more and more—now forming flocculi or adhering masses.

14551. XLI. No deposition—fine ruby colour and in its original state apparently.

14552. XLIII. The preparation XXV D shaken up—part poured into a clean bottle and then a little solution of common salt added, without more water. It changed at once and from red brown by reflected light became violet brown. By transmitted light it was also changed, being far more of a blue violet than before—the salt having altered it, just as it before did the ruby fluid ( ). Made the preparation XLIII. In two hours, *now* the colour nearly gone because the deposition is quick and the deposit itself is insignificant in appearance, so great is the aggregation of the gold by the mere effect of the salt solution (14593).

14553. No. 184, which is double deep gold on glass—heated by blow pipe in spirit lamp flame ( )—its reflective power is diminished and rendered very moderate. By transmitted light, instead of a dark green, it is of a pale brown colour when seen

on paper behind or against it. No green remains in the parts sufficiently heated–they appear light brown, dark brown or opaque, and the darkest parts are those which reflect most and have most gold. A wooden point drawn across can remove the gold from the parts where the green colour is nearly gone–also from parts where it is quite gone, but at still more highly heated parts the gold is scarcely removed; and where a fold makes the metal double or triple in a hot part, all the thicknesses are joined and adhere as one thickness.

14554. When a brown part is pressed by a convex surface of agate, the pressure alone, if enough, can render the place green. Burnishing with the agate and saliva also renders the place green. A steel burnisher also makes places green, but it seems liable to scratch up the gold. The agate pestle top and oil also renders parts green by pressure and rubbing, but not so well as I expected.

14555. The green appears only on the thick parts and not on the more translucent parts or thinner parts, i.e. when viewed against the bright lamp flame; by weaker light, as that of white paper, the thinner parts are greenish when the thicker parts (of the granulation) are dark. As the heat is applied, the thicker parts of the leaf are the last to lose their greenness, and by rubbing, etc. afterwards they are the first to regain it.

14556. No. 187 is deep gold, heated, etc. ( ); little or no trace of green left–probably heated on both sides. The agate pestle by a pressure rolling improves the reflexion of the place and makes it green by transmitted light. Dry agate rubbing does the same but less effectually where the heat was highest. Oil seems to interfere with the development of the green, as if a certain holding on the metal by the burnisher were required. When platina foil was laid on the gold and a steel burnisher applied over it–no greening effect was produced.

14557. No. 189. New gold leaf–much heated–burnishes well by the agate pestle–becomes fairly green and by day light a good green. I think there may well be differences of glass; probably one not too fusible or soft will be best.

14558. No. 191. Green gold, i.e. IV ( ). Well heated so that no green or blue colour remained–the glass softened. When burnished it became blue by transmitted light. The burnishing

improved the reflexion on the upper surface of the gold as if it laid particles flat; but the surface *against the glass* suffered no corresponding change; it was a fixture. The gold surface felt rough as if the glass beneath was irregular or the gold irregular. The thick parts of the flocculi surface are green by strong lamp light flame—but dark by a weaker light. There is a certain golden reflexion at the back surface of the burnished places, seen when the gold and glass are held in a peculiar oblique position.

14559. No. 192. White Gold leaf—well heated. When cold had a corrugated surface and pearly appearance. Will not burnish with dry agate—much friction and tears up. Neither with saliva. The surface is wrinkled and granular, and the tops of the particles rub off.

14560. No. 193. Silver leaf heated. The metal surface quite granular, rough to the nail like a file or sandstone—the agate just rubs off the tops of these prominences.

14561. I begin to think the burnishing only flattens down and *spreads* the gold which has gathered up by the heat—in fact that it thins it as beating it would do.

14562. The Phosphorus film 106, 111, 112, 113, have been heated and now make Nos. 180, 181, 182, 183. On none of these is the gold so fixed that a wooden point cannot remove it, except where 3 or 4 folds of the films have given a body of gold, and there it sticks hard. Some beautiful phenomena appear in some of these.

14563. No. 180. A fine film—thicker at one side—and perhaps has been more fired in one part than another. Where most fired a slight pressure of the wooden point does not displace the gold but makes it a beautiful green—*very beautiful*, pure and regular—the reflexion at the streak places is lessened in brilliancy and becomes copper red and that on both faces of the gold. A rub with the finger causes the change to green. Wiping with the edge of a card is excellent. None of the clear spots or cracks originally in the film are changed in form by this treatment—the gold is not displaced except a hard wood rub be employed.

14564. No. 181. Is not as 180—it rubs off with a wooden point at every part and does not change to green.

No. 182. Rubs off easily and is as 181—the film is thin.

No. 183. This is a thick film but it rubs off. Agate rubbing or rolling produces very little green, if any.

14565. No. 180 again. The agate pestel rolling on it makes it green—or agate rubbing makes it green—a steel burnisher makes it green.

14566. Nos. 95 and 96, two films heated formerly (14488, 94), rub off with wooden point and produce no green.

14567. Nos. 103 and 104 are parts of the same film as No. 180 (14532); they have been well heated and made No. 194 and 195. No. 109 was a like film (14533); it has now been heated and made 196. Now 194 is as 180 and changes well to green, and I think best at the most heated place.

14568. No. 195—half of it is even now green to a certain degree; the other half is purplish or ruby with rather a sharp outline of division. Wood rubbing improves the green part and converts the purple or ruby part into green also—just like No. 180. All the gold is fast and the most heated part is the best.

14569. No. 196 (14567). This changes and becomes greenish by rubbing with wood, but is not so good as the two last films 194 and 195. The gold rubs off easier. Still, the green streak is given here and there—the quality is present. A wipe or rub with a finger brings out a greenish tint. Look at it by daylight.

14570. Six drops De la Rue's gold on a surface about 3 inches square put over phosphorus vapour as before and left there 40 minutes, then taken of[f]. In taking off, it run up much, being carried by the water towards the glass, but the plane parts were film of good character. When laid on glass to dry it made four specimens numbered 197, 198, 199, 200.

14571. Eight drops of the solution on a like surface left for an hour—then taken off; very much wrinkled and broken up: the film had tendency to wet on both surfaces and the edges hung down in the water. Specimens numbered from 201 to 204. Both these preparations had been levelled from time to time to make the fluid between the glass and film flow from place to place.

14572. Six drops on—not disturbed during the hour they were left. At the end, taken off and made specimens from 205 to 210. The film ran up and was corrugated as before.

14573. Six drops on with more phosphorus—the gold film formed rapidly. Being left 30 minutes, it was taken to the washing basin. The film wetted and went under with the glass and would not float, being moistened doubtless with phosphoric acid. It looked like a good film, and rolled about, curving with the water. It appeared to be slate purple by transmitted light. When touched by a silver tooth pick—it clung to the point as if immediate adhesion occurred. When two places touched each other, they clung together. The film seem[ed] to be very metallic and golden. Some very corrugated specimens were laid on glass and are numbered from 211 to 217.

14574. Whilst these films (all of them) were over the phosphorus and forming, they tended to contract, so that the edges of that first formed were after a time, about half an hour, fully the $\frac{1}{5}$ of an inch less all round than the edge of the solution place.

14575. A piece of double deep gold on glass had a drop of oil put on the metallic surface; it made scarcely a sensible difference either in the reflected or transmitted light. When reduced by wiping to a thin film, the reflection was redder on the oiled face than on other parts of the face or on the other side—the effect seemed due to the action of a *thin plate* of oil.

14576. The phosphorus dishes of the 20th (14570) were put at that time into the washing water and left as before (14535). To-day the liquor is of a fine ruby colour; there is a little film on the surface but no gold about the particles of phosphorus at the bottom. It was divided into two portions and bottled; one was left unaltered and marked XLIV, the other had a little solution of chloride of sodium added to it, which turned it to blue violet, and then was marked XLV.

14577. As to the gold films of Thursday (14570), Nos. 197–200 had a dead gold reflexion. By transmitted light the tint was a slate violet, a little greenish. At the folds of the film it was the same tint much deeper and approaching and reaching darkness. When a single film was much inclined the tint changed to purplish slate. 198, 199 have certainly a greenish tint and where there is a thickness of two, or when two films are looked through at once, the tint is a real dull or brown green. Nos. 201–204 had the same colour and character but were thinner, I think, than the former series. Nos. 205–210 were generally like the last but not so thick or so green, more slaty in colour. Nos. 211 to 217 were of a slate violet colour–were golden–and were perhaps the thinnest of all.

14578. Nos. 199, 200, 202, 203, 208, 209, 211, 212, 217 were heated in the spirit lamp flame (on the back of the glass) as before (14486, 94, 14586).

14579. Placed some of the Ruby and violet deposits on glass. For this purpose resumed XXXI (14474), which has at the bottom of the flask a most beautiful deposit, black or opaque in the middle and a fine ruby at the edge, the whole extending not more than a sixpence; the fluid generally had the ruby tint. The fluid was poured off until about a teaspoonful of liquid was left with the deposit. This deposit being shaken up, mixed with the fluid, though like settled starch it clung rather to the bottom, and when mixed up it made a fluid opaque to reflected vision and looking like diffused brown gold, but translucent and by transmitted light of a *fine violet colour*. Some drops of this thickened liquid were placed on four glass plates to be numbered 218, 219, 220, 221. Then the flask XXIII (14483), being De la Rue's violet fluid boiled, was taken in hand. The fluid had a violet tint and

the sediment was violet—the latter was almost too small to collect. On pouring off the liquor the sediment left tended to clot a little, but being diffuzed it was disposed of on two glasses to-day. These are to be numbered 222, 223. These six glass[es] were put on a plate—a capsule with caustic potassa placed over them to dry them and the whole covered in by a glass cover. The fluid portions were left with the same numbers as before, but the separated deposit of XXXI was made XLVI.

14580. The theory of undulations should either account for all the phenomena of light, including reflection, absorption, transmission, etc., or else must be accepted as only an imperfect and partial view of part of the full and true theory to be developed.

14581. To point out or lead to a knowledge of what it either cannot explain or has not explained, is quite as important for the progress of knowledge as to establish what it can do.

14582. Hence the importance of dealing with dimensions so far within the limits of an undulation as those that can be given us with gold and metals, where all the phenomena of reflexion, transmission, and absorption happen within limits far less than those of an undulation.

14583. May perhaps hope here to change one undulation into another, and so *change* one ray or colour into another, that problem which I have so long had in mind.

14584. A common ray of mono coloured light is supposed to consist of lateral undulation in *every* plane parallel to the course of the ray. Are the recurring nodes at equal distances from the source of the light or varying distances? I.e. do they all occur in the same advancing plane, or are the nodes of some rays in one plane and some in another? In the latter case, the nodes would occur in every plane (transverse to the ray), and if in every plane and in every azimuthal direction, how is it that they do not neutralize each other?

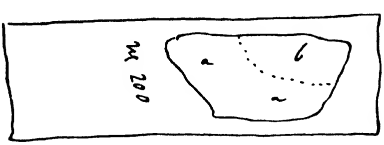
14585. Fresnel, I think, makes light from common objects to have this mixed character—but that from the stars, etc. to be in floods succeeding each other, but each flood regular—and thus explains Young's interference when star light is used.

14586. The films heated on the 22nd (14578) were to-day examined. They all transmitted more light than the unheated films and had changed the tint, so as from greenish to become violet. The reflexion was not much altered. When examined for the greening effect, none of them came up to the effect in No. 180. Generally speaking, the gold did not adhere to the glass so well and yet I think that No. 180 must have been heated less than these specimens.

14587. No. 197 is the unaltered or unheated film—a touch of wood, finger or card brings the gold away—there is no adhesion. Agate rolling pressure brings up all the gold, which adheres to the agate, shewing a fine reflective surface, but I cannot tell if it has been rendered green. Must use a plano convex lens for that. Nos. 199 and 200 are parts of the same film heated (14578). Wood, card or finger rubs off the gold, which though it adheres more than with No. 197, still comes away—but a trace of greening appears here and there in the tracks. Agate rolling pressure developes green colour very fairly, i.e. in the place where the film is single, but not where it is doubled by folding over in corrugations. At these places also the reflexion is much improved, so that the golden reflexion and the green tint appear at once. As to this improved reflexion, No. 200 presented the following effect: looking through the glass at the gold, by reflexion it is golden at *a*, *a**, but at *b* it appears purple or red brown and not so metallic as at *a*—it seems to be in closer contact with the glass there and does not rub off so easily. On the other or golden side, the reflexion is all alike and dead. When the agate pressure is given over *a*, *a*, it increases the golden reflexion, whether the gold is looked at directly or through the glass; but when the pressure is applied at *b*, though the green appears and the gold reflexion is improved on the metallic side, it is really diminished on the glass side and looks a little less metallic there than before.

14588. No. 201 is an unaltered film and just like 197 (14587); Nos. 202 and 203 are the same film heated. Card and wood rub remove the gold easily. The agate rolling pressure developes a very good green in some places where the film is single—and in some places the green appears imperfectly where the film is double.

* [14587]



14589. No. 206 is an unaltered film and like the others (14587); Nos. 208 and 209 the same heated. No. 208 resists card a little and in one or two small place[s] card greens the gold well. Agate rolling pressure produces the green, but the film is apparently not thick enough—a good pressure is required. The folds of the film do not become green. No. 209 is a crumpled heap—the card does not remove it but also it does not green it. Agate rolling pressure does not produce green. Doubling or folds seem to oppose greening. I think that a thicker film is wanting, but that doubling a film is not equivalent to a thicker film.
14590. Nos. 211, 212, 214, 217 are parts of the same film. No. 214 which has not been heated is like the others (14587). No. 211 gives negative results like 209. Nos. 212 and 217 have the gold removed by the card and no greening produced. Agate rolling pressure produces more or less of greening at single film places, but not at the folds. Very high heating does not seem to be good.

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14591. The ruby fluid XLIV of the 22nd (14576) remains unchanged. No. XLV, being the same fluid with a little salt (14576), has been entirely precipitated and no colour or gold remains in the fluid when poured off. The deposit seems small and forms on the bottom of the bottle a good dingy violet film—not adhering. This deposit, being diffused through water, gave a dingy violet fluid, muddy—and was put into a glass cell having four flat glasses at the bottom—to be numbered 224, 225, 226, 227.
14592. Some of the ruby (14576) had a little salt added—it became violet and was put into a cell with a glass under. No. 228.
14593. The precipitate by salt XLIII (14552), being red deposit with salt, now made violet. The liquid was colourless; there was a little deposition on the bottom of the bottle but it was hardly visible. I think the aggregation has gone on since I last looked at it (14552).
14594. The purple sediment XXXIV (14493) has now clotted together as a cobweb mass and clings where part touches part: it appears to have such a perfectly clean metallic surface as to adhere where it touches.

14595. XXXI. As the sediments on 218–221 (14579) are very poor, I shook up the fine deposit XXXI and placed three large drops on three glass[es] with the caustic potash as before (14579), to dry. Will make them 229, 230, 231.

14596. A Glass of the ruby fluid XLIV had a drop of ammonia added to it to alter it a little; it was then made XLVII. I want a purple fluid that will not settle too fast (14608).

14597. Heated films, greened by pressure (14587), were now reheated. No. 199, on being heated in the spirit lamp flame, lost its greenness at the pressed places before the blow pipe was applied and at a comparitively low temperature, perhaps 600° F. or less. Still, I heated with the blow pipe as before the effect of the pressure as to reflexion disappeared at the single films though not at the folds. When cold and many hours after, the rolling agate pressure restored the green at the pressed places. Hence most likely a condition of the particles and not a consequence of mere thinness—a condition of restraint and polarity. No. 200 presented the same effects even more beautifully. No. 203 presented the same phenomena. Reheating the parts not pressed did not seem to hurt them.

14598. No. 180, which greens so well by pressure, was 106. So took 105 and 107, which are parts of the same film, and heated them, making them now Nos. 232 and 233. No. 232 was well heated as before; still, when cold, card could easily remove the gold; however, at one corner the gold adhered and there the card made the gold green and well. Agate rolling pressure did not take up the gold from the glass and it made it beautifully green. No. 233 was less heated than 232. Card easily removed the gold and so did the finger, but the agate with rolling pressure made it fine green in every part. The films are good and uniform and were made by me (14532).

14599. No. 212 (14590): the heated gold was moistened with a solution of chlo. gold; dried and heated, it gave faint green by Agate rolling pressure. A clear place on 212 had a drop of chlo. gold put on it—evaporated and heated. This by Agate rolling and pressure gave an imperfect feeble green—shewing the phenomenon though imperfectly. There is a fine though faint ruby tint on the glass round the place of the drop at parts where the

vapour had carried and deposited chloride of gold and gold; it is the ruby of finely divided gold.

14600. As to the four deposits 218–221 (14579). Two of them were left unchanged, 218, 220; they were very imperfect films—very irregular—and seemed to contain very little gold—the reflexion was very poor, the transmitted light was gray violet. Nos. 219, 221 were heated and were violet rather more. Card did not rub off the gold and gave a tinge of green, but the particles seem very separate: must examine them by the microscope.

14601. The two deposits 222, 223 (14579). No. 222 left unchanged. Still is slate violet by transmitted light, and dull golden by reflected light. No. 223 has been heated and is more golden by reflected light and more ruby violet by transmitted light. Card friction makes it green and agate rolling pressure also, improving at the same time the reflexion; but there is so little gold that the effect is very feeble.

14602. The green transmission and the bright reflexion seem to belong to each other; perhaps the pressure, etc. only places the faces of the particles in a common plane—or perhaps gives faces to the particles.

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14602[1]. The glass with ammonia, XLVII (14596) had not altered in tint or settled by to-day, but was ruby as before.

14603. No. 200 (14600) has again been heated and again lost its green transmission and its conjoint reflexion—by rerolling and pressing it with the agate, both again appeared. No. 233 was also reheated and lost its greenness and much though not all of the ref[l]exion it had gained by pressure; it has a general golden reflexion and a violet transmission. Pressure applied now reproduced the former effect.

14604. The three drops of deposit 229, 230, 231 (14595) being dry were resumed. They make a very poor shew, looking like dirty spots, but the metal reflects a golden light especially in sunshine or a strong light. By transmission the light is a dirty violet. By rolling pressure with agate or glass, the golden

[1] 14602 is repeated in the MS.

reflexion come on strongly, but the transmission may have a little of green but is very dark; and the gold is probably in particles more separated and thicker than those which transmit green. No. 231 has been heated: the metal is very little changed in its appearance or character–it does not adhere to the glass–card easily sweeps it off–by the agate rolling pressure the golden reflexion came up highly, but the green transmission was very imperfect if any–these films are very unequally spread–and therefore very irregular in their different parts–231 was broken up.

14605. Resumed the four glasses of Nos. 224, 225, 226, 227 (14591) prepared from the deposit of violet fluid XLV. They formed not continuous but granulated films on the glasses, which whilst wet was dingy violet both by reflected and transmitted light–but as it dried the gold reflected a brown bronze colour, considerably metallic, whilst the light transmitted was still the same. In sun light the reflexion was much better from the glass side of the gold than the other, because the glass gave a plane reflective surface to the gold. The Gold was very loose on the glass. Agate or glass rolling makes it highly reflective, but the particles are too large to become translucent by the pressure. No. 227 was heated: it became much more bright and golden by reflected light, and also let more light through by transmission, the particles having drawn together. Strong rolling agate pressure gave great metallic reflection and by degrees a translucent effect just appeared in the parts most pressed–a dark green or purplish green being transmitted.

14606. No. 228 (14592), being the direct violet deposit from a ruby fluid with a little salt, is a finer film than the former, but in all other respects like them. The fluid had almost entirely settled, but there was a little violet cloud. Whilst wet the film was of a slate violet colour both by reflected and transmitted light, but as it dried it became golden yellow with bronze dullness by reflected light from the gold surface, and with a darker greener appearance the same effect occurred on the glass side of the gold. Then by transmitted light it was greener than when wet. The change depended upon the medium, water or air or glass, in contact with the gold and affecting the action of light at the surfaces of contact; the change was considerable and remarkable. When

dry once—moistening with water caused the film to float and not to wet—moistening with alcohol did not return the tint to that of the metal wet with water. Agate pressure gave reflexion—but no sensible greening.

14607. The ruby and violet fluids contain very little gold—the effect is due to the great separation.

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14608. Ruby fluid XLVII (14596) with a little *ammonia* had undergone no change of colour or appearance. Added several drops of sat. solution Mur. Ammonia to it and left it.

14609. Three glasses of the Ruby fluid XLIV (14576) were prepared and a little chloride sodium added to each, the proportions being 1 : 4 and 16 of salt, but the largest quantity very small.

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14610. No change had happened either in the ruby fluid XLVII (14608) or in the three glass[es] with small quantities of salt (14609); a drop of brine was added to each; the three small glasses, 3 c.i. each, turned blue at the bottom at once and were left to settle. The large glass XLVII changed but little. Next day added three drops more to this large glass—did not stir up any of them.

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14611. These four glasses examined: the bottom part violet, the upper part ruby. The violet part has begun to fall to the bottom—the ruby part not. Left to precipitate.

14612. A little dilute proto mur. tin made acid by M.A. and a solution of gold containing excess over the tin were mingled together—it became red brown, then darker and darker and more ruby coloured, being at last very like the phosphorus ruby fluid. It continued to improve for some time and is made No. XLVIII. Common salt does not turn it violet or blue or change its colour; the tin seemes to have made the divided state of the gold stable. A portion of this ruby or garnet fluid was taken and excess of dilute proto mur. tin added—it darkened at once to a red brown as if all the gold were now at once taken down. It is made XLIX.

14613. Examined many of the fluids by a ray of sunlight, the

fluids being in bottles, glasses and cells. The ray was allowed to fall on to them, and that light which passed through was received and observed on white card board. Besides that, much light was reflected by the particles in the fluid and this made them look opalescent. Such results of reflected light were well obtained when the light was collected into a cone by a lens and that sent into the fluid. The cone was well defined in the fluid by the illuminated particles. Where this cone came near the surface of the glass or fluid, the light reflected by the metallic particles there was yellow and golden, but when the particles were further in, the light they reflected had to be transmitted through the ruby or other fluid and then the yellow light was modified. When the lens was so adjusted that the cone passed thrgh. the glass and fell on a card board placed to receive the rays, then the transmitted light was seen there. The lens cone was very beautiful in all the cases, and always shewed distinctly the presence of the particles in the fluid.

14614. Ruby fluid XLII (14535) was somewhat thick to the eye and only a small proportion of light passed finally thrgh. the quart bottle—the phenomena were very good. The ruby fluid XLIV (14576) was clearer—the transmitted ruby was very fine and a beautiful cone of illuminated particles was produced. The ruby fluid XLII (14535) was the same. My old ruby fluid XXVII (14321, 445) was weaker, but had all the same characters.

14615. De la Rue's purple fluid XXVIII (14447) gave a violet transmitted light, but the cone of rays shewed the diffuzed particles and also that their reflected light was yellow. XXX B is a fine ruby fluid in a glass (14549, 50), but just as the others in its nature and constitution—the reflection of particles near the surface was very golden. XXX A (14549) is a very fine ruby fluid and the golden reflexion in the cone excellent. XXV L (14546) is a very good ruby, in constitution like the rest—the cone excellent. XXXI ruby, very fine; is as XXV L.

14616. XLVIII, or the Garnet fluid above (14612): the colour is deep and the light much obstructed—the lens gives the cone and it is yellow like gold at the entrance surface, but the particles seem very fine as yet. When this fluid was diluted, it much more resembled the ruby fluids, and the cone shewed in better manner

the particles and their golden character. In 6 hours after, the purple body had separated and fallen in flocculi, and the supernatent fluid had only a very pale yellowish brown tint. Must see if it contains any gold in solution, and what is the habit and constitution of the precipitate where there can be only a very small portion of tin.

14617. The Gold deposit XXV D (14547) by the reflected sun light alone or with the lens was yellow or brown yellow, and shewed a dense gold mud. As the glass was moved and the particles rose in streams and striæ, they shewed well, and then the cone of rays looked very beautiful, full of veins. The light that went through on to the paper beyond was purple violet or ruby according to the density of the muddy stream, but that which was reflected in *all directions* but directly across was the brown yellow of the gold. The preparation XXXI (14550, 79, 615), i.e. the deposit from boiled ruby fluid, presented the same phenomena.

14618. The fluids in tube were examined chiefly by a small lens. XXI, which is boiled ruby fluid (14343, 443, 4), had settled so much that top looked clear of particles, but the cone of light shewed yellow particles even there. When stirred up it shewed the beautiful phenomena of the former preparations, but the settling particles are I think gradually aggregating. XXV in tube was clear at the top, purple at the bottom with a gradual change between. The cone of light shewed particles every where—there were few at the top and the reflected light from them was green, like that of the heated film greened by a card—*there is probably a common cause in the two cases*; the particles here must be the finest possible. On carrying the cone into the lower parts, it was good to see how the reflected light increased on entering the sensibly violet shades of fluid. The tube XXIV has stood in the sunshine a few minutes and the currents formed have carried up the lower parts, but the effect is generally the same as in the tube XXV. Only the *green* was *not so clear*. The Sun and the lens shewed beautifully the currents of moving particles where no effect was visible by ordinary light.

14619. Some ruby fluid (XLIV, 14576) was placed in a square glass cell—a fine ruby tint was transmitted. The lens and cone of

rays gave a good effect and shewed abundance of floating, reflecting gold. Added common salt—the fluid became violet at once and I think transmitted more light than before. The cone of light gave I think a browner reflection, as if the gold more aggregated and like brown gold—but there was abundance of reflected light from the interior. Some hours after, this gold had separated and fallen to the bottom and the liquid was clear. There were a few air bubbles in this and the Cassius vessel (14612), but I had used common water from the cock for their dilution and there is air in that water (14642).

14620. The transmitted light of gold leaf is green. When thinned by chlorine or by cyanide of potassium, the green continued as long as there was gold, becoming paler and paler. This was illustrated by Nos. 69, 71, 73, 77, 82, 83, 121, 122, 126, 129. No. 73 and 83 were thinned until the green was nearly gone, and yet a thin transparent film remained; as these were thinned by chlorine, the film may have consisted in part of chloride of silver, that metal always being present—but 126 and 129 were made very thin by cyanide potassium, and then no silver is left as chloride.

14621. Gold film obtained by V. Electricity on zinc, No. 65. This film may be transparent—the transmitted light was not green but bluish perhaps, like No. 41; but the lens and cone of light did not give good effect of transparency. No. 66, being double films, was the same in character.

14622. Palladium, No. 46. Grey reflexion and transmission. No colour. Platinum, No. 85. No sensible trace of the metal.

14623. Silver thinned by N. A., No. 165—is translucent, passing a grey light. Copper thinned by Cyanide potassium, Nos. 140, 145, 148, is translucent in parts and these of a bluish grey colour; but as it was thinned on the surface of cyanide of potassium containing some silver and gold, a film of these substances may have been deposited on it.

14624. The heated Gold leaf (   ) transmits green rays where not too much heated; where more heated—more light passes but the colour disappears. Yet when highly heated, a yellow is transmitted, as if the gold or perhaps the silver in it had stained the glass. Nos. 93, 94, 97, 98. Heat with silver leaf produces a like

loss of reflection, etc., but the yellow stain is greater than with gold Nos. 174, 176, 179, 186, 193.

14625. *Films of Gold by Phosphorus.* The transmitted light is almost always blue violet, not green. It was so with two superposed films—with three, No. 41—with four, No. 42—with thirteen films, No. 51—the colour remains the same when the intensity is graduated. Many of these, as No. 42, resemble the colour of pale gold nearly. But some of these films do give a transmitted light that partakes of green, though dull. Nos. 232, 233, 197.

14626. When such films are heated, both the reflexions and the transmission is changed and perhaps improved. Nos. 95, 96 when most heated are most changed. In many cases the beautiful phenomenon of green transmission by pressure is produced. Nos. 180, 182, 199, 217.

14627. The ruby fluid evaporated left a blue or violet film, No. 84. The deposit from ruby fluid No. 220, when looked at by reflected light, shews a different effect at the surface next the glass to that at the surface next the air. As the glass and gold do not adhere except by ordinary contact, this alone shews the action of two neighbouring particles on one ray of light, for the glass influences the ray which the gold ultimately reflects or transmits.

14628. When a little gold mud is added to fluid, as water, it makes it ruby—when more, it renders it violet.

14629. Preparing red or ruby fluid by phosphorus and solution of gold, and then adding salt to cause separation, gives a very finely divided gold free from tin.

14630. There is no going back from violet to ruby in the order of colour. The ruby fluid cannot be made concentrated above a certain degree at once—by causing separation of particles and then using the mud, it may be obtained very concentrated.

14631. All the phenomena look as if they were the proper results of affections of fine particles.

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14632. The four glasses (14611) are still in the same state. The glass with excess of proto Mur. tin, XLIX (14612), is very brown and with no precipitate.

14633. The Cassius purple, XLVIII (14612, 6). A little of it with pure muriatic acid was not altered in colour at first, but the flocculi broke up and the gold became diffuzed and gradually became less ruby or more violet—it then forms a fine deposit. Salt does not affect the Cassius purple.

14634. A little solution of proto chloride of tin did not change the violet fluid (14447) to ruby or towds. it. On the contrary, a little of the same substance tended to change the ruby fluid to violet slowly. This change soon stops, if any effect at all is produced; the ruby does not become ultimately blue violet.

14635. The green of the very clear fluid XXV (14618) induced me to filter some ruby fluid XXVII, and some violet fluid XXVIII through filtering paper to separate the coarser particles and leave the finer diffuzed. The ruby soon tinted the filter ruby and of a good colour. The violet did not give a sensible tint and seemed to pass more freely. After a time and several returns of the liquids, the latter retained very little diffuzed gold. Had not sun light—but lamp light and a lens shewed suspended particles. Marked the ruby filtering paper (14641).

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14636. Continued the observations of the plates and deposits in the sun (14620, etc.) Nos. 218, 219, 220 are deposits made by a drop of the mud deposit from the ruby fluid (14579, 600). No. 219 has been heated. They make but a very poor show on the glass in sunshine or not; the light transmitted has a little violet tint, and that reflected when in the cone of rays is golden; but there is very little effect considering the quantity of gold there compared to its effect when diffused in fluid. Nos. 222 and 223 are like deposits from the mud of a violet fluid (14601), the latter being heated. The light reflected is yellow, that transmitted dull violet in 222 and rose tint in 223; but the effect of heat has been great in favouring the transmission of light. No. 229 is a deposit like 218 in character, but more abundant and has the same transmission.

14637. No. 224. The deposit from ruby fluid by common salt—reflects golden yellow—that which passes goes thgh. holes and separation, but is perhaps a little green, very dull and dark, as if

the particles or massed particles nearly opaque. The same heated, No. 227, has then a very gold reflexion instead of the slate colour of the former, i.e. by diffused light; the light which passes is probably through holes only. No. 228: same thing in a finer state, unheated; has like 224 a poor golden reflexion, a general slate reflexion and a tint of purple in the transmitted light.

14638. The No. 229, being particles of Gold from mud on glass, reflects dull gold on metal side, but a striking green on the glass side, very like that seen in compressed gold leaf or film. This must shew conjoint and compound action of the gold particles and the glass particles on a ray of light between them. The effect is very striking.

14639. Remember that where the gold films are burnished and the green appears, the diffuzed reflexion disappears, so that one has reason to expect that the diffused transmission becomes at the same moment more direct.

14640. No. 54. The Sul. Iron precipitate gives no distinct transmitted colour.

14641. The filtered ruby fluid (14635), after many filtrations, had fine diffuzed particles which the cone of sunlight found out, but their reflected colour was golden, not green. The same was the case with the filtered violet fluid—the remaining particles were golden. The tubes XXIV and XXV being looked at again, the former was golden but the latter a beautiful green as before (14618).

14642. The blue deposit, all from ruby fluid and salt (14619), when examined by sun light, lens and cone, shewed *no particles* left diffused through the fluid. The blue or dark deposit at the bottom of the cell formed a film dull violet by transmitted light and, whilst wet, also by general reflected light. When the sun focus was thrown on to it wet, a dead yellow was reflected; but the heating rays were active and the spot quickly dried, and then there was a great increase of reflective power, and then the reflection by diffused light was much greater and golden—not dark purple—much as if the film had been heated; but ordinary drying did the same thing and wetting again did not remove this character, due probably to aggregation—the light transmitted remained unchanged in colour. By shaking the fluid with the wetted particles about, they stick together, and if any spots stick

to the glass the others stick to them, and at last they all seem to disappear, except the small adhering spots. This is an effect of welding or adhesion of the clean metallic gold.

14643. The experiments (14612, 6, 14632, 3, 4), XLVIII and XLIX, with sol. of Gold and proto mur. of tin, were resumed. The mother liquor of the first Cassius which formed contained no solid particles that the light cone could find. Nor did the second mother liquor—for both these contained excess of gold in solution. On adding a little proto chloride of tin to this second mother liquor, it became brown, shewing gold there. After many hours it still looked like a solution, but the cone shewed particles which could reflect.

14644. The Cassius precipitate XLVIII, being washed in water, gave a good violet precipitate in flocculi. Some of the particles diffused through water and subject to the Sun and lens gave the cone, and the particles seemed as luminous and light coloured as gold, though by diffuse light reflected the Cassius looks purple. Muriatic acid added to a little of the Cassius breaks up the flocculi and gives a smooth blue violet fluid, looking like the violet gold fluid. The cone of rays transmitted blue violet rays, and reflected bright golden light. Nitric acid produced a like effect but not to the same degree—the cone effect was the same as before. Sulc. acid slowly produced a similar effect—but less strongly. Caustic potassa produced no apparent change. The light cone shewed the same phenomena as the other cases.

14645. No. XLIX (14612). Gold with great excess of Pro. chlo. tin—has to-day arrived at a fine ruby colour and looks at first like a solution, but in the light focus (being diluted) the cone shewed particles reflecting golden or strong light.

14646. Gold leaf on sol. cyanide of potassium and light passing through whilst it thinned: it never lost its green tint or acquired a ruby or any other colour—the green once conferred is permanent until heat comes. Whilst dissolving, the parts too thick to transmit light became thinner and transmitted green light, whilst the still thinner parts became insensible.

14647. A Gold film deposited by Phos. vapour, No. 115, and having a dull olive greenish slate like tint, was put into chlorine gas diluted—it was gradually thinned but did not change in colour.

After a time, it was floated on a solution of chlorine and dissolved away; but the tint did not change in colour, only in intensity.

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14648. Went to see M. Petitjean's process of silvering glass—which was according to the description he gave me. When the solution was put on to the glass, it took about $4\frac{1}{2}$ fluid ounces to stand on a square foot of surface. By putting white paper under the glass, enough light was sent back to shew the colour by transmission of the thinnest film, and that was fine slate purple, like that seen through pale gold. The reflexion was scarcely sensible at these places yet and by the time it became distinct, as is the manner with gold, the transmitted light was scarcely any. It would seem as if there were a great deal of light extinguished by a film of a certain thickness, for it is almost opaque and yet scarcely reflects any light. Surely the full reflexion of silver must depend on the action of many particles at once. Sometimes floating films in small pieces formed—have a very fine surface, not dead but brilliant and continuous. Then these were opaque. When so thin as to give scarcely any reflexion, they transmitted the same blue light as the former (14723).

14649. The first solution being on, after about 15′ the deposit begins. It is beautiful to see how the dabs of the cotton or the intermitting rests of the roller used to wet the solution appear, shewing how small a circumstance influences the deposition—by further deposition these became covered up and disappear. After the solution has been on sufficiently long, the reflection from the back of the silver (the dead surface) is at deep angles yellow; when the coat is thickened and increased, it becomes a brown violet. When the solution is poured off and the plate washed, as the back dries the colour changes much from difference in the light reflected.

14650. He scraped off silver from a place as large as a shilling, and proceeded to mend it, removing first the varnish by turpentine, and then cleaning the place with rotten-stone, cotton, etc.; the silver bore hard rubbing on the back and polished well there—it was really very tough. The mending was not complete before I came away.

14651. The XLIX (14612, 32, 45) was of a very fine bright rose ruby this morning, but had settled as a ruby jelly containing I think much oxide of tin, and the liquor above was colourless. Being agitated, it had the appearance of a beautiful bright ruby cloud or mud. Added Muriatic acid freely—the change was not immediate, but after some hours there was a purple or violet deposit and the mass of solid jelly was gone.

14652. A strong solution of Phosphorus in Sulphuret carbon was put on film 198, and a thin glass put over it to flatten the surface. The transmitted tint was but little affected; the reflexion was less than before, but of the same character.

14653. Gold leaf pressed on platinum wire gauze and heated over the spirit lamp. When heated round to a certain degree the gold leaf began to wrinkle and shrivel up, tearing apart at the weaker places, and this went on until the gold attained the fuzing point; but long before that, it had contracted into portion[s] scarcely visible, seeming to have disappeared, as on the glass, but having only contracted into denser masses. As long as the gold leaf was visible it was green—it did not loose its colour of transmitted light by the heat. A dull red heat was borne by parts without loosing the leaf state and they still remained green. On the other hand, there was no part that remained as leaf that was not green—there was no production of film of any other transmitted colour. When Gold leaf was put into olive oil and raised to the highest temperature the oil could bear, the metal leaf did not contract or change in character or form and it was always transparent and green—the colour here seemed even more regular and finer than in air—in fact all interfering reflections were evaded.

14654. A leaf of high gold dissolved in a little solution of chlorine —filtered—evaporated to dryness in a water bath and dissolved in water. Five glasses were prepared and numbered. A solution of phosphorus in sulphide of carbon was also prepared. One drop was put into glass CI, and one tenth of the solution of gold having been added to 8 oz. of water, the whole was put into glass CI and set aside. Glass CII contained at first 3 drops of the phosphorus fluid and the same proportion of Gold and water as the last, with the addition of 10 drops of M.A. pure. Glass CIII contained 3 or 4 drops of phosphorus solution—the same gold

and water as the last and 50 drops or more of pure Muriatic acid. Glass CIV contained 2 or 3 drops phosphorus fluid, three tenths of the whole of the gold, 8 oz. water and 20 drops of Mur. acid. Glass [C]V contained 1 or 2 drops phosphorus fluid, four tenths of the gold, 8 oz. water and no extra muriatic acid. These were all put on about 11 o'clk. At 1 o'clk. there were signs of ruby in CV, and perhaps in one or two of the others (14662).

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14655. The films, solutions, etc. examined by polarized light. First used a candle flame, and Nichol's prisms for polarizer and analyzer; placed the following specimens between and found no trace of action on the polarized ray, whether the film was vertical to the ray or inclined at any angle to it or to the plane of polarization, i.e. in any azimuth to it: Nos. 39, 41, 42, 54, 65, 66, 84, 96, 98, 117, 122, 126, 129, 130, 135, 138, 139, 140, 145, 148, 172, 179, 197, 218, 220, 222, 224, 225, 226, 228. Certain films seemed to produce a depolarizing effect when much inclined; these were Nos. 46, 51, 69, 71, 77, 82, 83, 108, 118, 121; but in fact they also were indifferent, and the effect was that produced by the inclined glass when its inclination (not being either in the plane of polarization or the transverse plane) was in the plane 45° from the plane of polarization on either side. It was the glass and not the gold that produced the effect.

14656. The heated films were better examined by a diffuse light from a globe lamp polarized by reflection from glass—for then, by turning the glass on which the film was laid and turning the analyzer, the black lines could be made to traverse all parts of the glass; and as then the film of gold shewed *no effects* in these parts, so it was concluded that it was indifferent. In this way Nos. 95, 94, 97, 174, 180, 182, 186, 199, 217, 219, 221, 223, 227, 229, 230, 232, 233, appeared to have no depolarizing power.

14657. No. 165, being a silver film thinned by nitric acid, depolarizes a little. I am not clear at present to what this effect is due—can it possibly be a degree of crystallization by dissection—or a sub compound of silver?

14658. Several of these films were placed before the polarizer but with no effect.

14659. The three ruby and violet fluids, XL, XLII, XLIV, were placed in the course of the polarized ray; they exerted no influence upon it.

14660. So films beaten, deposited, heated, pressed, thinned, deposited by electricity, etc., etc., all agreed in shewing no action on the polarized ray. This result is rather remarkable in a body which has such high powers of action on a ray of light in other directions.

14661. Some of the Ruby deposit XXXI, i.e. an opaque drip of it and its water, was put into a tube and absolute alcohol added to it. There was the same effect of reflexion and transmission as with water, but the gold aggregated and so disappeared much–let it settle–poured off the alcohol; powder dark and heavy–added oil–heated–powder aggregated still more but preserved its character–it is unchanged by such treatment except in the degree of aggregation.

14662. The glasses of different strengths (14654) are to-day as follows. No. CI–a little colour but very poor, and as deposit chiefly. No. CII–a little colour, purple violet, in the fluid, but scarcely sensible. No. CIII–no sensible effect. No. CIV–very little effect–only a film on the top of the solution. No. CV–a good fair purple violet liquid. When a portion of it was diluted 5 times with water–it then had a tint nearly as deep as No. CII. The process does not promise much.

14663. I made a weak solution of gold, so as to have pale yellow colour. I prepared two glasses with phosphorus nearly alike. I put ½ oz. of the solution into one of the glasses–and numbered it CVI. I put ½ oz. of the solution with 5½ oz. water into the other glass, CVII. This about 10 o'clk.–the stronger solution began to act at once and produce ruby fluid–left both to go on.

14664. Provided some gold terminals and a voltaic battery of 30 pr. Grove's plates, then brought the terminals suddenly together and separate, so as to have a momentary deflagration. Did this upon and between glass plates, so that the deflagrated gold should be deposited on the glass in films more or less graduated. The gold terminals were sometimes the ends of gold wire, or two sovereigns, or a wire and a sovereign. The deposits had all a like character, but varied one from another as might be expected.

They are numbered from 234 to 243. The continuance of the dischge. between two sovereigns did not increase the deposit but rather seemed to blow away that previously arranged.

14665. Much of the deposited gold was easily removed by rubbing with the finger or a card, but near the focus of action the glass was often melted and the gold strongly attached. The general character of the deposition was that of the phosphorus films, but the golden metallic reflexion was in many parts much stronger. Many parts which had a very strong reflexion still transmitted light and that was coloured violet or blue violet—passing at times into ruby and rose tint; but the slate violet was the most abundant. Still, the outer limits of the deposits where the finer particles had been driven were often of a ruby tint. There was no sign of green—it was the ruby or the violet or mixed shades of these.

14666. When the discharge had been continued for a short time with sovereigns, which could bear this action, there were evidences of the effect of heat on the particles, and at these times some good ruby tinted deposits were produced. This ruby adhered to the glass more than the violet; still, a rub with a card edge could often remove it.

14667. Pressure by agate was able in favourable parts of these films to increase or rather produce a golden reflecting surface where it was not before, and also to cause transmission then of a green light (No. 240), though the green was not often good. Expect to do this better with deflagrations made by the Leyden battery.

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Conduction of these films, leaves, etc.

14668. Employed our large but sensible Galvanometer—and at first one pr. of Grove's plates. These being connected, terminals of fine copper wire were arranged the ends of which, being bent into a round loop, pressed downwds. with a little spring force, and so could be placed on any part of a given film on glass and be moved by friction from place to place. I found this gave very good and sufficient contact with the films.

14669. *Beaten metal leaves* conduct well; so did all those that

had been *thinned* down, as 70, 73, 80, 83, also 128, 136, 138, 146, 170 and 173, including Gold, silver and copper. When very thin, conduction was interfered with, as was shewn by 83 (where the metal is deep gold thinned by strong Mur. acid (14432))—the green tint is almost gone and the metal itself, but still it conducted, but more feebly.

14670. No. 54. *The deposited particles* from precipitation by Sulphate of iron—conducts.

14671. *Films of Gold by phosphorus, unheated.* The regular, uniform and freely translucent films did not conduct; this was shewn by Nos. 2, 5, 7, 9, 35. On the other hand, very thick solid and golden looking films, as 20, 25, 56, conducted like gold leaf. No. 13 is a film with doublings across it: the uniform film did not conduct, the doublings did. The film 205 conducted pretty well even in parts very translucent and thin as to quantity of gold, and Nos. 108 and 118 had the like comparatively good conducting power; these films had been a long while over the phosphorus basin, so that one might expect a certain continuity of film in their cases.

14672. The films of Palladium, Nos. 43, 46, conducted, for a feeble current passed. The like film of silver, No. 47, being transparent, conducted. The Electro deposited film of silver conducted well, No. 65.

14673. *Effect of heat on beaten gold leaf.* The specimens 97, 177, 185, 191, being gold leaf heated, conducted even in the parts where the signs of the gold were nearly gone, but not so well as the unchged. leaf. No. 176 seemed to have quite interrupted places, for some parts or places conducted well and others were much worse. The White gold, 94, and the Silver, 179, also conducted: in all these cases the conduction was more free than I expected.

14674. Certain of the *phosphorus gold films* have *been heated.* Of these, Nos. 95, 96, 194, 195, 232, 233, did *not conduct* sensibly. No. 196 conducted a little—but feebly. Several films have folds, as 181, 200, 196, 208—here there was no conduction at the simple unfolded film, but more or less by the masses or veins formed by the duplications. No. 202, though folded, did not shew conduction; the film was thin. Where parts had been greened by

agate pressure there was no additional conduction sensible. No. 183 conducted both in the unheated and the heated part, but it is a comparitively thick film. I think that, on the whole, heat impairs the conduction of the films, when they possess the power before hand.

14675. The deposits from Voltaic deflagration, No. 240 (14664) do not conduct in the dark part—even where pressed and caused to give metallic reflexion.

14676. Now employed *four pr. Grove's* plates for the films that had shewn no conduction with one pair. The unheated films Nos. 2, 5, 7, 35. The heated films 96, 194, 195, 232, 233, did not conduct. The heated folded films 181, 196, 200, 208, were as before, and also 211, 202 and 204. The film 9 has one end heated and the other not—the heated end does not conduct but the unheated end does, and as the wire was moved over the surface there were even sparks and deflagrations, the current then clearing until the end was moved to another place, when a like deflagration occurred; but this did not occur unless *both* wire ends were on the unheated part of the film, shewing that the heat had injured the conduction, and doubtless by separating the particles in consequence of their contraction and aggregation.

14677. Yesterday the fluids CVI and CVII examd. (14663). No. CVI was much changed and very deep in colour. No. CVII was also changed and of a good colour. I added water to CVI, so as to make it 6 oz., like the CVII. It was somewhat deeper in colour and of a tint also somewhat different. To-day: the two fluids are still of different tint, but very nearly alike in depth, for CVII has gained since yesterday. The comparison seems pretty good, but it will be best to begin with a solution strong enough, and at least stronger than CVII, and dilute after the ruby fluid is formed.

14678. Nos. CI, CII, CIII, CIV and CV (14654, 62) are as they were on the 9th and are of no value. I added a little ammonia to CIII to neutralize the excess of acid, but expect nothing further now.

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14679. Nos. CI, CII, CIII, CIV, CV (14678) still the same, so dismissed them. Nos. CVI and CVII (14677) were as they were—alike

in depth—no film on the surface—no change in their condition. Put the fluids CV, CVI and CVII together in a flask, added a little salt only, and boiled the fluid for nearly an hour—it became more purple in colour, but not blue—and being then numbered LI, set it aside to settle. It was at this time a very good purple of considerable depth.

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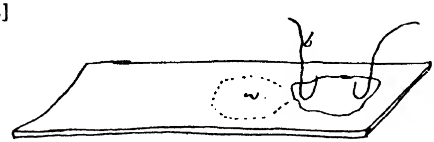
14680. To-day LI (14679) was still purple and there was no settling—left it still; a portion of it was tried with more salt and became quite blue violet, so that the quantity of the salt is important.

14681. Proceeded to make Gold films by phosphorus as before (   ) and arranged to try the conduction of the films for electricity. Six drops of De la Rue gold solution (   ) was put on the glass plate over the phosphorus, and after 15 minutes was transferred to water—the film was as dark as I wanted it, but weak and ran up together a good deal.

14682*. In order to try the conduction, the same arrangement as before (14668) with a single pair of Grove's plates was employed, but the two terminal wires were curved at the ends. Then the film, being taken up (after ten minutes repose on the water to wash it) upon a plate of glass and floating on the water on the glass, was placed under the terminals, the water serving as a cushion to hold it against them. The films conducted and very well, and the Galvanometer was deflected. To prove that this conduction was through the film and not through the water, the latter was drawn out at one side of the film, as at *w*, and then when the terminal *b* was placed in the water there was no deflection of the galvanometer. When both terminals were in the water and within the $\frac{1}{10}$ of an inch of each other, there was no conduction. Or, if when both were touching the gold film, a card was drawn across the gold so as to separate it, there was *no conduction*. When the film was drained and dried on the glass, it conducted freely.

14683. The specimens of this film on glass are numbered from 244 to 249. Nos. 244 and 245 gave the same results as to conduction. Nos. 246, 247, 248 had the water drained from beneath them—then solution of borax introduced—this drained off and

* [14682]



the films then dried; after which they were heated, as was also 249, which had no borax. The unchanged films 244 and 245 were greyish green by transmitted light and had a certain amount of gold metallic reflexion by reflected light. The like film 249 heated had about the same reflexion but transmitted much more light than before, as is the case when heat is applied, and that light was rose violet. Nos. 246, 247 and 248 having borax, are by reflexion much as 249, but in certain positions there is a fine ruby effect added here and there. No. 249 gold is easily displaced by the card rub, and there is no greening. No. 246 with borax is also easily displaced, and there is no greening. No. 247, which is more highly heated, has the gold fixed at one side but not at the other–and where it is fixed the card friction makes it quite green by transmitted light. No. 248 rubs off and does not green. Agate pressure applied to Nos. 244, 245 picks off the gold. Applied to the other four, it makes them all finely green, but 249 adheres in part to the agate.

14684. Another film formation with 8 drops of gold solution on the glass plate. It was left over the phosphorus for 22 minutes and ran up much when transferred to water. Specimens were collected on glass numbered 250–254. No. 250 conducted as before, whilst the fluid beneath did not conduct (14682). The films were about as the last in general character. The four first had borax introduced, and being dried, Nos. 251, 252 and 253 were heated; after this they rubbed up without greening and I think the heat is not enough. No. 250 sticks a little and all of them, i.e. 250, 251, 252 and 253, green well by agate pressure.

14685. No. 254–when from the water, had strong Muriatic acid placed under it, but the film did not seem to be dissolved or changed. On a former occasion even gold leaf seemed to be dissolved by Mur. acid–but it was of course an alloy.

14686. Another film formation, with 4 drops gold solution, over the phosphorus only 5 minutes. But the film was very thin and weak and seemed to have no strength at all on the water. It forms Nos. 255, 256, 257, 258, and the three latter had borax solution placed under them and when dry were heated. This film did *not conduct*–it is very thin–it has but very little reflective power and it transmit[s] a greenish gray tint. Nos. 256, 257 and

258, being heated, had about as much reflexion as before, but now the light transmitted is a fine ruby tint, so that when laid upon white paper the film has a very beautiful colour—just like that of the finely divided fluid (ruby). Card rubbing does easily remove the gold, but it turns the transmitted ruby tint to a *blue violet*; and Agate pressure does the same thing—only there is so little gold that scarcely any result is obtained, especially in the point of reflexion. These fine films are very interesting.

14687. Another film formation, but added Sulphuret of carbon to the phosphorus in hopes it would make a stronger and more consistent film. Used 5 drops of gold; over the phosphorus for 5 minutes, for by the appearance, the film seemed to form much faster than before. I found that in this case, when transferring the film to the water, it was better that the glass should be nearly flat rather than inclined much. This film conducted feebly but the fluid beneath not sensibly, so it is intermediate between the films of the two last formations (14682, 6).

14688. The reflection of this film, Nos. 259, 260, is better than the last—the transmitted colour also deeper but of the same kind. Nos. 262, 263 and 264 had borax solution placed under them and when all were dry, these with 261 having no borax, were heated. 261 then presented a dull purple tint, but the other three gave the fine ruby tint. Nos. 263 and 264 were more heated at the extreme end than elsewhere, and they shew the change by the heat, first from the Greenish grey to ruby and then from ruby to yellow almost, where the gold seems to have run more together. No. 261 easily rubs off by card and the others are not much more fixed. Where the Gold adheres in the ruby parts it is changed to violet. The agate pressure makes the usual change in all the parts, but more in some than in others.

14689. Parts of this film were submitted to the action of Muriatic acid, Nitric acid, Sulphuric acid, Sol. potassa and brine, but none of them appeared to act upon or change them.

14690. Six drops of the gold on the glass plate over much Sulrt. carbon and phosphorus for 15 minutes. The result gave specimens from 265 to 271. These were I think thinner than the last but like them in character. Nos. 265, 266, 267 had no borax. Nos. 268, 269, 270, 271 had borax. Nos. 267, 268, 269, 270 were heated.

List of Objects continued from pp. 2946, 2990, 3004[1].

244, 245. Gold films by phos., 14681, 3.
246, 247, 248. Do. . . . with borax, 14683, and heated.
249. Do. . . . no borax, but heated, 14683.
250. Do. . . . 14684, with borax.
251, 252, 253. Do. . . with borax and heated, 14684.
254. Do. . . no borax, 14684, 5.
255. Very thin film, 14686.
256, 257, 258. Do. . borax and heat, 14686, fine ruby tint.
259, 260—medium gold film, 14687, 8, 5238.
261. Do. . . . . . heated, 14688.
262, 263, 264—with borax, heated, 14688.
265, 266, 267—films, no borax, 14690.
268, 269, 270, 271—films with borax, 14690.
267, 268, 269, 270—films heated, 14690.
272, 274—film not heated, 14691.
273, 275, 276—films, borax and heated, 14691.
277 to 300—films and rings from surface of standing solution, 14694, 707, 11.
301 to 318. Do. . . . . 14697, 11, 5238.
319 to 340. Deflagrations of gold wires, 14699, 708, 12.
341 to 354. Deflagrations of silver wire, 14702, 9, 13, 5262.
280, 1, 9, 302, 5, 7, 10, 14—gold films, heated, 14711.
321, 7, 34. Gold deflagrations, heated, 14712, 5240, 58.
343, 7, 50. Silver deflagrations, heated, 14713.
355, 6, 7, 8. Thick gold films by phos., heated, 14715, 20.
359–367. Gold films by phos., much borax, heated, 14717.
368–375. Gold films, thick and thin, 14721*.
376–383. Do. . . . . 14723*.
386–393. Gold explosions in oxygen, some heated, 14835–41, 5261, 3.
394–399. Do. . . . . air, Do. . . . 14835–41, 5261.
400–405. Do. . . . . hydrogen, Do. . . . 14835–41.
406–410. Green films by phosphorus, 15065.
411. Jelly with gold sol.—blue, 15076, 343.
412. Do. . . . gold surface, blue, 15077, 343.
413. Do. . . . very little colour, 15078.
414. Jelly alone, 15079.
415. Jelly and gold sol., fine ruby, 15079.
416. Do. . . . . . blue pale, 15080, 343.
417–424. Ruby jellies dried on glass, 15155, 342.
425. Beaker ruby for rotation results (15166).
426, 427. Cells for blue and ruby fluids (15168, 15211).
428 to 432—films of Jelly dried, 15228, 32.
On to p. 3172[2].

[1] See pp. 17, 55, 67.

[2] See p. 207.

All assumed the ruby colour. Where the card did not wipe off the gold, it changed it from ruby to green, and agate pressure did the same well.

14691. More sulphuret carbon added—6 drops gold, over it for 32 minutes—the result is veiny and not very thick. I think that after the first film is formed the action goes on slowly. Nos. 272 and 273 had no borax. Nos. 274, 275, 276 with borax. Nos. 273, 275 and 276 heated. Agate pressure effect very good at 273, but not so good on 275 and 276, which are thinner. Where the card does not rub off the gold it changes the tint.

14692. Sat. sol. nitrate silver over phosphorus: very poor film in half an hour. Employ solution of the ammonic nitrate.

14693. The phosphorus basins were now put each into the weak solution of Gold resulting from the washing of its film—covered over with a plate and left all night and for more than 15 hours, that the Ruby fluid might be produced.

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14694. This morning but little formation of ruby fluid had taken place, but fine films had formed on the surface of both basins; and as minute fragments of phosphorus had floated, so around each had been formed a fine thicker film as large as a sixpence, a shilling, etc.; the central part being clear of gold, then a ring of fine rich gold and that fading off into the thickness or thinness of the general film. Here most beautiful gradations of thickness were obtained, and where the gold was thickest it had its full resplendency of reflexion. Took up many specimens of these rings and of other parts of the film, which was excellent in character, and these number from 277 to 300.

14695. The general film is very good and stiff and breaks up as if solid: the formation of the rings is curious, as if a film of gold were reduced at the phosphorus particles and then pushed outwards, causing the aggregation in the ring. The body of the ring is of a fine green and it is not very dissimilar in intensity from one edge to the other, though the ring may be 0·25 or 0·33 of an inch broad between the inner and outer diameter. It does not present the gradation that I expected. The golden reflexion at

the green part is very striking. All the transmitted tints are comprized between the green and the slate violet or greyish green.
14696. Left the basins and liquids for 24 hours, to see.

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14697. This morning took other specimens of films from of[f] the fluids (14696)—they are numbered from 301 to 318. The observations before made apply to these. Nos. 315 to 318 are on paper. Drew off the solutions (ruby but weak) by a syphon—put them together—added salt until they were blue violet and left them to settle—made this preparation LII.
14698. The purple stain of organic matter appears to me to be purely reduced gold.
14699. *Gold wires exploded* by the Leyden battery. Numbered from 319 to 340. Here we obtain the general tints obtained by other processes—supporting the conclusion that these other process[es] yield divided gold and not compounds of it. In many parts the gold is deposited with its fine reflective lustre, but at these places it is quite transparent and offers very fine tints of ruby, green, violet, etc. Nos. 322, 324, 327, 328, 329, 330, 332 offer very fine greens by lamp light. With the highest charges the division is the best. Nos. 333, 334, 335, 336 present cases of a two fold wire and they are much beneath those made at the same time with a single wire.
14700. In some of the cases, as 327, 329, 330, 331, 332, there are results of a two fold course, all the Electricity not having gone by one wire in its place, but a part of the charge directly across; and from the appearances it seems that this second part has been carried across by gold spread abroad by the first discharge. See 331 and 332.
14701. Much of the Gold is merely dispersed—some is found an inch from the place of the wire. Much of this is easily wiped off—but near the place of the wire it is much fixed and there has a ruby tint. The glass is often fuzed here—scaled off sometimes—and pulverized if the plate is not strong.
14702. Then silver wire was exploded and gave Nos. from 341 to 354. In the first place, striking to see how dark a deposit it gives, i.e. how much light is *absorbed* by the divided silver. In

some case[s] very little is reflected and very little transmitted, and that as a dark brown. The deposit is very loose except where fuzed into the glass, and much of this loose deposit has a deep chocolate reflexion in the air. Transmitted colour brown chiefly—violet frequently in small quantities—little green now and then.

14703. A weak solution of gold, 4 drops De la Rue solution to 60 oz. of distilled water, was put into a large basin and when all was quiet, a plate was put over it having a piece of phosphorus from the sulrt. carbon solution on it but not touching the solution of gold, so that the film might be formed slowly on the surface of the fluid—then this was left in a dark quiet place. Some of the same solution was put into a glass with a piece of phosphorus beneath the surface for a comparison (14704).

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14704. The expt. above (14703) a failure, for covering plate touched the fluid. Also the phosphorus oxidized in the air and became covered with phosphatic acid and damp: there was no film. By the evening there was some reddening—however, left the fluid with two little fragments of phosphorus moistd. in sulphuret of carbon floating on the surface and covered it to see if any film will form by tomorrow. The fluid in the glass with the piece of phosphorus at the bottom was ruby round the phosphorus (14715).

14705. No. LI (14679, 80) is a beautiful and comparitively deep violet fluid which as yet shews no signs of settling at the bottom and is equal throughout. By the lens and cone of light it is easy to illuminate the golden particles within and they shine as fine gold. Upon looking at them so illuminated and employing a strong eye glass, I thought I could even distinguish the particles.

14706. No. LII (14697) was a blue fluid. There is no deposit but the colour is much gone and it is only in large masses that the fluid appears of a very pale blue green colour. But a lens and a cone of sun's rays shews the particles in the fluid of a yellow colour, golden, and then when looked at through a strong eye glass many of them are easily seen floating about. They have aggregated and become individually larger and they are larger than in No. LI,

and hence the disappearance of the general colour as the metal aggregates.

14707. The gold films (Nos. 277 to 300, 14694) examined by day light. Every part that reflected, reflected yellow light, and when the sun's rays were thrown by a lens on to No. 277, 281, 284, 292 in a dark box and the reflected rays received on white paper, this was very evident. The transmitted light is from slate blue in the thinnest part to green in the thickest and most brilliant part of the rings, i.e. most brilliant by reflected light. There was no tint of ruby or tendency to it—but in other cases the ruby has appeared in small veins (   ).

14708. The Gold deflagrations (14699), being Nos. 319 to 340, were examined in day light. Every part of the gold film reflected yellow light and when the cone of sun's rays were thrown on to it in a dark box, this was very evident, for the lights sent back from the glass alone and from the glass with film on it were very different. The transmitted light was very various, from a rose on the outside by a green or green violet to ruby at the place of the wire and heat. Nos. 321, 7, 8, 30, 1, 3, 4, 5, etc. shew the outside rosy tint due to the finest particles. Nos. 323, 9, 30, 37, 8, etc. shew good violet. Nos. 322, 4, 7, 8, 9, 30, 2, which by lamp light exhibited very fine greens, now shew these places as very fine violets.

14709. Silver deflagrations (14702), Nos. 341 to 354. In many parts of these much silver is deposited, looking bright and metallic by reflection, especially on the glass side—in one case it is even golden; but the reflection from the general surface is very dark—brown black. When the sun's rays in a cone are sent on to it, the light which is reflected back is not golden but white. In fact it resembles that reflected from black paper with the same cone, only there is more of it with the silver. The transmitted light is brown, greenish brown and fine violet.

14710. I think that in both the Gold and silver deflagrations there is much loss, absorption or destruction of the light which falls upon them, and most in the silver cases.

14711. Applied heat (gas urged by air) to some of these gold films (   ) and amongst them to Nos. 280, 281, 289, 302, 305, 307, 310, 314. Where the film had been regular and thin, i.e. with

only a little golden reflexion, there the reflexion was not much altered, but the transmitted light was fine ruby or violet ruby; in most places, this easily rubbed off with a card, no borax having been employed; but where it adhered well to the glass, the card rub changed the colour, and so also did agate pressure--the reflexion character is also deepened in tint and changed at the same place. Where the Gold was thicker, as at the rings, there is much change: thus the chief bright golden reflexion is destroyed, the Gold having drawn together into particles apparently, as with the gold leaf heated–the gold does not adhere to the glass in these places, for a card removes it; the light transmitted there is no longer green but nearly without tint and far more abundant than before. Agate pressure develops a little of the green tint on these parts, but the effect is very imperfect, for the surface has not that regularity which seems best for the development. In former cases, when there was mere corrugation of the film, that seemed to interfere much and to a distance with the production of the greening on the side parts–apparently an equal film and a regular pressure is required, perhaps implying the necessity of consecutive particles in the plate or film altered by the pressure, and then even a card or finger touch is enough.

14712. Gold deflagrations heated, being Nos. 321, 327, 334 (14699, 708). In these cases the Gold reflection is not much interfered with, except that where the gold was in quantity it is lessened and the metal is more dead. Neither is the transmitted ruby tint in the place of the wire and most heat particularly altered, but the violet parts are generally changed into ruby, the change being great. Where however there was most gold, i.e. near the line of dischge., the gold has disappeared, as in the case of Gold leaf, not being as a thin uniform film on the glass but drawn up as I think into independent globules, and a lens focus sent on to it shews it there, though by common reflected or transmitted light the gold would seem to be away.

14713. The silver deflagrations (14702, 9). These give a line of silver burnt into the glass and on each side a black film–which near the line is nearly opaque and thins off in shade in the distance. On the metal side this film has little metallic lustre; on the glass side more, its surface being made level and reflective on that side.

But when agate pressure is applied on the metal side, the full reflective lustre of silver comes out there and the transmitted light there increases and become[s] a dark blue (14723); further off, where there is no sign of metallic reflection on either air or glass side, the agate pressure produces the metallic reflection on both sides, and at the same time *diminishes* the transmission of light there, the colour being then a dark blue. It is very interesting to see how strong white reflexion and good blue transmission is produced, as in No. 352, where before there was scarcely a visible trace of metal. It shews well that the deflagrated silver is metallic and that the particles are translucent. Now when some of these are heated, the dark silver deposit becomes of a yellowish brown hue, much changed in appearance from the former and corresponding to the change of silver leaf heated; but yet when agate pressed, the same reflective lustre and the same transmitted blue tint appear as with the unheated specimens: see Nos. 343, 347. By the application of the heat, the black smokiness of the silver deposit is changed to a more milky and dead silver appearance. The particles are I think aggregated. The finest particles seem to destroy light, i.e. absorb it, etc. very greatly. In the focus of the lens the white heated silver is much more brilliant than the black unheated film.

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14714. The deflagrated gold (14699, 708) was not affected by Nitric acid—nor by muriatic acid. The deflagrated silvers (14702) were soluble at once in Nitric acid.

14715. The fluid (14704) was a little ruby but there was no film worth consideration (this was yesterday). A couple of oz. of the fluid was put into a small dish—one drop of the de la Rue Gold solution added to it and stirred, then one little piece of phosphorus was put floating on the surface and another little piece placed at the bottom. A ruby diffusion soon occurred round the submerged piece. Also a ruby film formed round the floating piece thus, then round that an outer blue tint with golden reflexion—the film is forming. Ruby streams or striæ fall into the liquid from the floating piece, and the formation and falling down appear to be intermitting—the descent is not all direct from the

phosphorus particles, but from a ring around about the eight[h] of an inch off or more. As it proceeds, the effects are very beautiful. To-day the dish presents a fine metallic film on the surface round the phosphorus and a fine ruby fluid below, but there is no gold next the phosphorus; all is clear to a distance of the eighth of an inch, as if emanating phosphorus vapour had pushed the forming gold film outwards. I took up the thick part of the film on four plates—introduced borax beneath—then heated them by blow pipe and numbered them 355, 356, 357 and 358 (14720).

14716. A little piece of phosphorus was left floating on the cleared fluid to see if any gold left. Next day no signs of film.

14717. Yesterday 8 drops sol. gold into large basin of [ ]— and two or three small particles of phosphorus from Sulrt. Carbon left floating on the surface. In half an hour, circular films were seen forming round the phosphorus and some faint beautiful striæ of ruby falling. To-day there was a good film on the fluid— thickened in the rings round the phosphorus. The phosphorus does not appear to consume rapidly and very small particles answer the purpose. I took a large portion of film off on a large glass and the rest on smaller glass. The ruby formation was but small—a little gold was still found in the solution by proto muriate of tin. The Glass plates and film had borax introduced and afterwards when drained they were left wetter than on former occasions, so that the effect of more borax might be observed. When dry they were heated at the blow pipe, and form films Nos. 359, 360, 361, 362, 363, 364, 365, 366, 367.

14718. Decomposed Acetate of Lead and Acetate of Zinc by the Voltaic battery, employing Lead and Zinc as the Positive electrodes, but the finest crystals I could procure were opaque when examined by an eye glass.

14719. A mud of gold in distilled water XXXI gave no sensible conduction with 4 pr. of Grove's plates unless the electrodes were large, and then the effect was the same when the gold was away (15053).

14720. The four films (14715) Nos. 355, 6, 7, 8 had changed by heat as usual. Many parts had been over heated and passed the amethystine ruby character into a browner tint, and these did not produce the change to green by the card friction, and the agate pressure only made a little change. The portions not so much heated had a fine violet or amethystine colour by transmitted light and these became finely green either by the friction of the card or the pressure of agate.

14721. Nos. 359–367 (14717) are generally like the last–all ruby–fine. They did not change tint so well by agate pressure as by card rub–and Nos. 361, 364, 365, 366, 367 were very good in that respect. Where the glass has been only little heated, moisture and a slight rub remove the gold; but where it is well heated, though not too much, there the gold is fairly fixed.

14722. Yesterday eveng. 12 drops of De la Rue gold was put into distilled water in the great basin as before and two little fragments of phosphorus were left floating on the surface at 7 o'clk. P.M. This morning–very fair films have formed, though with this small quantity of phosphorus, and the action is still going on–for which purpose it was left undisturbed (14721*).

14723. Mr Petitjean (14648, etc.) sent me two pieces of glass with deposits of silver on them, graduated from end to end. At one end little reflection and little obstruction to light–at the other, perfect reflection on the glass side and quite opaque. The reflexion was much finer, i.e. more regular, on the glass side than on the metal side, that being comparitively dead. In passing from the thinner to the thicker parts of the film, the transmitted light became dark, i.e. blue dark, the colour blue being very distinct. A card removed the silver where it was in small quantity–but agate pressure applied there produced a more reflective surface, interrupted the transmission of light, and the light transmitted was blue–like as with the silver deflagrations (14713). No doubt this effect and the effect on gold are of the same kind.

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14721*. The film put on to form 2 days ago (14722) was taken off this evening in eight portions numbered 368–375. They varied from a moderate grey film to an opaque film, the latter very

brilliant and able to peal up from the glass in separate leaves. As the films thickened, they passed from violet or blue gray to green and then to opaque. I think the green of these films is different in cause to the green produced on the ruby heated films by the card or pressure. I left the thin loose disintegrated portions of the film floating on the fluid and put a larger piece of phosphorus on the surface (14723*).

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14722*. In reference to my ancient query, whether it is possible by any means to alter a ray by change of its number of vibrations or otherwise, so as to convert one ray into another without absorption (i.e. without an effect like that of Stokes or like that produced when a body absorbing sun's rays sends forth rays of a lower number of vibrations), I thought it possible that particles so small in relation to vibration dimension and yet so eminent in their power on light as is shewn by their reflexion effect, might perhaps produce some effect of this kind. This expectation indeed was one great stimulus to this long and as yet nearly fruitless set of experiments on gold. Therefore prepared an apparatus by which a thin beam of Sun light, about $\frac{1}{6}$th of an inch broad, was refracted by a pair of fine Bontemps' prisms and the spectrum sent into a dark room. By placing either a sheet or a slip of card board in the course of the refracted ray, the Spectrum altogether or any part of it could be received and examined; and then by interposing the films, etc. in the course of the ray near the screen, any change they were competent to effect in the character of the ray could be observed. Very many of the films were thus examined. According to their thickness and their tint, they intercepted more or less of the ray in which they were interposed, but I could not find that any one of them changed the colour of the ray which passed, or in that way produced the slightest indication of a change in the physical condition of the ray itself. The Nos. of the films thus employed were 8, 11, 15, 23, 35, 38, 42, 46, 53, 54, 68, 73, 80, 83, 89, 94, 95, 98, 127, 137, 145, 148, 174, 182, 195, 199, 204, 214, 218, 220, 222, 223, 228, 232, 234, 246, 249, 253, 265, 269, 319, 321, 334, 343, 346, 371—including all the varieties of film which I possess.

14723*. Took off the film left to form on the 19th (14721*) and made Nos. 376–383 of it. The film, though apparently thick by reflexion, was very loose and had little cohesion, and when taken up was poor in colour and effect by transmitted light. These films differ much as the time is longer or shorter, etc., and shew by that, with other things, that they are imperfectly aggregated and are not thin plates.

14724. Looked at a few of the Roman Numerals:

XXI (14618) tube. Upper half much settled, lower half not in 20 days (14739); other XXI tube boiled–settled to bottom–ruby.

XXIII (14479[1]) settled violet.

XXIV (14545) bottle–little gold here (14739)–XXIV in tube (14641) settled.

XXV D (14547) ruby settlement, plenty. XXV L (14615) ruby, half settled sediment at the bottom–the unsettled has been 20 days suspended (14739); XXV in tube settled (14739).

XXVII (14653[2]). Ruby–very little sediment–standing 15 days, 14739.

XXX A (14615). Ruby–both diffuzed and deposit fine, 14739.

XXX B (14615). Ruby–diffused and some settlement, 14739.

XXXI (14617). Ruby–diffused and fine sediment, 14739.

XLI (14551). Ruby–no sediment–is nearly 2 months old–mucus purple (14740).

XLIII (14552). No diffused gold–all deposited in little golden yellow [ ] 14740.

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14725. Today passed the prismatic ray (14722*) through various specimens of Ruby fluid–Violet fluid–Gold mud–Film No. 180 with parts rendered green by friction; but in all cases with the same results as before, i.e. with negation of any changing action on the coloured rays.

14726. Reflected the colour rays from the various films and also from the bottles of gold diffusions–the colour reflected was always the colour of the ray. Sent the coloured rays by a lens into the fluids so as to illuminate the diffused particles by the cone of

[1] ? 14483[1], page 51. [2] ? 14635.

light—the reflected colour was always the colour of the ray—no change.

14727. *Rose fluid* with little Sul. soda—boiled—changed in tint and on standing gave a violet precipitate. Some of same rose fluid with little Mur. acid boiled very long—all colour went and the gold was left as a brown gold deposit, very small in quantity and sticking well to the glass so as not to rince off. Another batch of rose fluid boiled from half an hour to 1½ hours—aggregation—by reflection was yellow brown, turbidness, by transmission ruby. Settled on standing 12 hours, which it did not before; but not more than ¼ of an inch from the top was clear of particles or colour. Another portion of the same, boiled for several hours and half the fluid evaporated, gave a result quickly settling, i.e. in 2 or 3 minutes, leaving the liquor very nearly without colour. The sediment was in small visible particles forming a bluish black deposit but which when shaken up gave no sensible colour to the fluid containing it. The result of boiling is aggregation, more or less in extent as the boiling is continued for a longer or shorter time.

14728. The deposit of Gold No.   , which appears blue black by common light because of the quantity absorbed is, when illuminated by the lens and sun's rays, of a golden colour in the focus.

14729. Some phosphorus from the sulrt. carbon solution was deposited on the inside of a small glass. Then 6 drops of De la Rue solution was put into about ⅔ wine glass of water, so as to make a comparitively strong solution, and that was put over the phosphorus, there being none of the latter at the surface of the solution—red formation immediately and much of it in 6 hours—it is Garnet coloured—deep—and sinks to the lower part of the glass.

14730. On the 26th, i.e. next day, the bottom dense part was drawn off by a syphon, LV, and the less changed portion left in contact with the phosphorus, LVI. On the 28th the colours of both glass[es] were still red or ruby, but there seems to be a gradual segregation of the gold from the liquid, for there was more appearance of muddiness by reflexion than before—they still transmit ruby but the liquid is I think more opaque than before.

14731. On the 5th May, the 2 portions of fluid were still deep red by transmitted colour but muddy by reflected light, LV and LVI. A drop or two of LV in water made LVII.

14732. Worked with the great Electro magnet excited by 20 pr. of plates and a polarized ray sent over the poles parallel to the magnetic axis. The poles were about the $\frac{1}{8}$ of an inch apart and then films, etc. were introduced and their effect, if any, when the magnet was excited, noted.

Films by phosphorus. Nos. 11, 15, 35, 42, 214, 371–no effect.

Deposits of gold. Nos. 54, 220, 222, 228–nothing.

Palladium. No. 46–nothing.

Gold, *silver* and *copper* leaves more or less thinned. Nos. III*e*, IV*a*, 68, 89, 137, 145, 148–nothing.

Voltaic deflagrations. No. 234–nothing.

Leyden battery deflagrations. Nos. 319, 341, 343–nothing.

Gold and silver leaf heated. Nos. 94, 98, 174. Nothing but what was due to the glass.

Deposited films heated. Nos. 95, 180, 182, 199, 232, 246, 269–nothing.

Electric explosions, heated. No. 321–nothing.

14733. Red fluid of 14 April 1856, in thickness of half an inch, caused rotation of the ray. But pure water did the same and in the same direction and amount.

14734. Fluid XXXI in different degrees of dilution tried, but always without effect, except that due to the water.

14735. The rich ruby fluid above (14731)–no effect.

14736. The polarized ray was sent *across* the magnetic axis and then a gold leaf–and also a tube with ruby solution (14731), but no effect.

14737. When XXXI was first looked at, the gold had settled to the bottom and given a deposit that was permeable to the ruby light of a candle, in all parts I think–but when it was stirred up, it was opaque to the same light–although it is manifest that far less gold interfered in the course of the ray in the second case compared to the first.

14738. Remember the angle of reflexion and that with metals, according to Potter, more is reflected at high incidences than at low ones.

14739. Observations at this date on the fluids:

XXI (14724). Sediment all at the bottom.

XX (14442). Sediment–all at the bottom.

XXIII (14724)–settled perfectly–violet.

XXIV (14724)–bottle–very little signs of gold–faint yellow.

XXV D (14724)–fine ruby deposit.

XXIV in tube (14724). No colour in fluid–no sensible deposit.

XXV in tube (14724)–fluid yellowish–fine ruby deposit.

XXV L (14724) in large glass–fine ruby deposit–but the fluid also ruby and uniform–must be very fine particles.

XXVII (14724). Ruby deposit now. Also ruby fluid uniform throughout–very fine particles–standing a month.

XXVIII (14653[1]). As it was–no deposit–de la Rue's first violet.

XXX A (14724) flask–fluid turbid and violet–fine violet deposit–think it has changed perhaps towards violet from ruby.

XXX B (14724). Glass–fluid ruby and uniform–ruby deposit.

XXXI (14724) bottle–dense deposit at the bottom–fluid clear and nearly colourless–faint rose. Small glass.

XXXI flask (14724)–ruby liquid–fine ruby deposit.

XXXII tube (14457)–violet as before.

14740. XXXIII (14550)–fluid clear–heavy dark–violet deposit.

XXXIV (14550)–fluid clear–deposit dark–clotted by mucus.

XLI (14724)–fluid ruby–deposit dark and clotted by mucus.

XLII (14614)–fine ruby fluid.

XLIII (14724)–fluid clear–deposit is brown gold–a little only.

XLIV (14614)–deep ruby fluid.

XLVIII (14644)–Cassius–as it was, purple deposit.

LI (14705). Violet fluid–very little violet deposit, 14845.

LIII. Is a general collection of violet deposit (14777).

LIV. Is some De la Rue violet, in a tube boiled long ago–the fluid is now clear and there is a violet deposit.

LV, dense ruby fluid (14731)–fine ruby deposit–fluid clearing.

LVI, weaker (14731)–fine ruby deposit–fluid clearing.

[1] ? 14635.

LVII, weak, very (14731)—here no clearing—fluid ruby throughout.

14741. Some of the fluids seems to me to become more turbid when viewed as by reflected light, but I am not sure.

14742. Went to Mr De la Rue with certain preparations, to examine them by his Microscope. Day time and day light.

14743. No. 124, deep gold thinned by cyanide potassium (14499). Examined by a power of 50 linear: the green gold is beautifully transparent, letting more and more light pass through and the green become[s] paler in the thinnest parts—by power of 700 linear the same thing. This specimen shews a bent line of red particles (14499) and these by the power of 700 are of a fine purple or ruby tint; the effect with a lower power is not an effect of contrast with the neighbouring parts. Seems as if a film of pale gold might be there. The form of this mark is just like that of a worm or a fine cotton thread or twisted hair; do not know what it is, but the gold is red there or ruby.

14744. No. 135. Silver leaf thinned by cyanide potassium (14500). By a power of 700, favourable parts appear as a dark purple—but Silver does not appear to be as transparent as gold in the same thickness, i.e. I cannot obtain thinner films of silver of a finer bluer colour, or freely transmitting purple or blue light. It would be good to obtain a very thin transparent film of silver.

14745. No. 93. Extra deep gold, heated (14486, 624). Microscopic power 220—aperture 60°. The unheated parts are of the usual fine green colour. In the heated parts the gold has run up more or less, especially at the edges of the pieces which by the heat contracted and rose up from the glass. Hence more glass cleared and more light let through and so the change by common examination. In the *middle* of a piece rolled up at the edges the leaf remained, and the colour seemed to have been changed from green to greenish blue. Many of the other parts were greenish blue, having a colour in the body of the particle different to what it was before heating.

14746. No. 189. New gold heated—burnished (14557). Power 700. The heated unchanged part has run up into granules by the heat, and these are purple and apparently translucent. The burnished places are generally green—the green parts shew

continuity in the microscope though often as dark as the purple parts—the purple particles become green by flattening. The particles which have run up are very beautiful and small; some were measured and were $\frac{1}{6100}$ of an inch diameter—others as small as $\frac{1}{45,000}$ of an inch in diameter.

14747. No. 191. Green gold, well heated (14558) and burnished. The heat appears to have caused retraction of the metal but the colour of the retracted metal is still deep blue as before heating. The burnishing seems to the naked eye to restore the blue where it had disappeared, but under the microscope the effect is rather to thin thickened parts and to cover more surface with the translucent gold. The retracted gold has left innumerable square place[s] from $\frac{1}{10,000}$ to $\frac{1}{20,000}$ of an inch across. Usually the gold seems to have drawn to the edge or edges of the square places, but occasionally it forms a globule in the middle. These cleared spaces are innumerable—very close together, remarkably alike in size and form—quadrangular, almost rectangular, and very sharply defined in the angles. They are very strange and the specimen must be examined again.

14748. No. 94. White gold, heated (14485, 7). Power of 550. The alloy shrunk up—edges recurved—colour deep purple in the middle of unmelted films—so also I think in the part not heated. The specimen is remarkable for the apparent disappearance of the alloy and the little obstruction to light at the heated place when examined by the unassisted eye.

14749. No. 183, dense phos. film, small (14564)—not heated part. A low power gave green parts passing into purple parts—the green parts continuous—the purple parts broken up. By a high power, 700, still the same effect; the greens were continuous—the purple parts broken up.

14750. No. 95. Phos. gold film, heated, pressed (14488, 529*b*, 66, 626, 74). With a power of 50 linear, every mark of the agate in rolling was shewn, of a green colour—the unheated part was gray—the heated unpressed part purple. The light transmitted appears about equal in the green and purple parts of the pressed and unpressed film. With an aperture of 90° and power of 700 linear, the part not heated is not resolvable into granules, but the part heated is granular even in the part greened by pressure. By

other observations the unheated part is gray and less granular—the heated part is purple and *more* granular. That the heated part should run up seems to shew that many of the particles must have been touching before hand, though they probably did not form a continuous film. By close observation the grey unheated part is resoluble into a mixture of green and purple striæ, the general effect of which is a gray.

14751. No. 195. Phos. film of gold—heated—green by streak (14568, 674, 6)—power of 60 linear—the purple and green parts appeared both granulated—the card mark is green—and this green is not an effect of interference but a positive colour. With a power of 700 directed on to the edge of the streak, it was seen that both tints were granulated, it not being altered by the force which had made the purple gold green. No. 180 was a like film with like treatment and gave the same phenomena—its references are (14537, 63, 5, 626).

14752. No. 322. Gold deflagration, unheated (14699, 708). Many large particles, then others, until the smallest not distinguishable, but forming green or green gray tint. Globules of melted gold sweeping over previous deposit produce a trace on that deposit and are fixed at the end, like so many comets and tails. The finest particles of the film not distinguishable—looks like a uniform tint.

14753. No. 321. A gold deflagration, heated (14699, 708, 12). The ruby character given to the gold is accompanied by granulation, as if the extra heat had caused touching particles to run together.

14754. No. 220. Deposit on Glass plate from ruby fluid (14579, 600, 3, 27, 36). The particles are violet in colour and of irregular sizes, but perceptibly distinct from each other.

14755. No. 222. Deposit on glass plate from violet fluid (14579, 601, 36). Mingled with the particles are transparent crystals, but these are probably the salt that was employed to produce the violet powder.

14756. XXV D. Gold mud from the ruby fluids (14547, 617, 724, 39) examined with its fluid, as a thin film between two flat glass [plates]. The gold particles were of different sizes and aggregated or clotted together more or less—there was no distinct form. Some of the particles appeared purple or violet, but on the whole

they were not transparent. Some of this mud was rubbed up in an agate mortar and then on glass, where it dried. Under the microscope transparent crystals or particles appeared, but these disappeared in N. A., leaving dark colour particles of blue or violet gold.

14757. XLIV. Ruby fluid (14576, 614, 740)—a drop by capillary attraction between two glass plates—some transparent particles floating about; but I do not believe we saw the particles which the sun's ray and a lens could shew were present. As yet the particles unresolved—would be a fine test object.

Ended with the Microscope for this time.

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14758. No. 227 is a deposit from blue fluid by salt (14637) and has been heated. It is very granular and the particles seem to be transparent by small lens—there is a rosy appearance at the side of the lens axis as if interference on rays passing between the particles was a cause of red or ruby colour; and where the particles had been agate pressed, the colour became dark green and the light transmitted much less than before, as if there it passed really through and not between the particles. Specimen needs further examination.

14759. LII is a large bottle of turbid ruby fluid. According to my notes it was at first a ruby solution rendered blue violet by salt (14697); next day it was a blue fluid not depositing (14706). A week after, i.e. on the 23 of April, it was all boiled and bottled up. Now it is a fine ruby fluid but turbid by reflected light, and it has a ruby deposit. If there is no mistake, it is a case of reversion of blue or violet fluid to ruby fluid and would be important.

14760. Put some of LII into a glass cell—ruby colour—added salt which sank to bottom and quickly made the lower half blue violet—the upper remaining ruby—more light was transmitted through the lower half—still, particles were shewn there by a ray through—gold there soon settled and in 4 or 5 hours most was at the bottom, whilst the ruby fluid remained unaltered—very interesting (14767).

14761. Employed my microscope and the highest powers, using the reflector with transmitted and also direct light, or reflected light from the sun. Employed the deposit at the bottom of flask XXXI (14724, 39), which by reflected light is as a brown mud liquor and by transmitted light of a deep purple violet, and used it in rather a dense state. Being put on a glass plate and thin glass over it, there was no sensible colour in so thin a film—I could not find any traces of gold particles in this form of experiment. Placing a drop on a glass plate, with no glass over it, there was evident colour in this drop. Being put on the Microscope stage with the drop uppermost, I could see no particles at first, but gradually flocculi formed on the glass by deposition, and these were of a dark purple grey colour by transmitted light—the flocculi were evidently aggregations of numerous smaller particles and I could move them about by a hair, but the small particles did not appear. It is probable that the glass surface had some effect in causing the aggregation of the flocculi particles—they seemed to be formed on it.

14762. I do not think that we have as yet seen any of the particles as they exist in the suspended red fluid—except as they are shewn by a cone of sun's rays or lamp rays.

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14763. Sent some of fluid XLII, ruby colour, with some of the deposit shaken up to Mr Brooke in a very clean bottle for Microscopic examination.

14764. No. XXXV. Ruby fluid (14761). A little of it with drop nitrate of silver shewed chloride silver, which after a little and by stirring clotted and took down all the gold with it. A portion of XXXV had some drops of N.A. added—the ruby turned blue violet and then gradually disappeared, the gold dissolving probably as it ought to do. A portion of XXXV yesterday with ammonia added. Today the gold had settled, very little, flocculent and unchanged in colour, or a little violet—the liquid poured off clear and colourless—water added to the deposit, then a drop or two of Mur. acid—the latter aided the aggregation into flocculi but did not dissolve the gold or cause it to disappear.

14765. A portion of XXXV had lime water added to it yesterday:

to-day the gold has gone down with lime or carbonate in large flocculi but the colour of the metal not changed. A drop of Nitric acid added dissolved the largest part (the lime, etc.), left the gold finely divided and blue violet in colour, gradually aggregating to give dull brown reflexion—but also gradually disappearing; and at last the gold dissolved, as it ought to do, chlorine and N. A. compounds being present (14778).

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14766. The auriferous chlo. silver above (14764)—a little ammonia to it—dissolved the chlo. silver and the gold was left finely divided, violet, as good as ever—boiling then caused it to aggregate but did not change its colour or character (14776).

14767. The cell (14760)—the fluids keep their respective places, but by diffusion the salt creeps upwds. and the ruby above is changing violet and gradually precipitating.

14768. $\begin{cases} \text{XXXVI is the deposit of XXX A.} \\ \text{XXXVII is the deposit of XXXI.} \end{cases}$

14769. LV, the deposit, was shaken up and with the fluid poured off from the phosphorus below, and then added to LVI and also LVII, and thus *one* muddy liquor was obtained and made LV*a*; and the deposit, which was brown yellow by reflected light and deep violet purple by transmitted light, was made LVI*a*.

14770. Portions of the deposits XXXVI, XXXVII and LVI*a* had absolute alcohol added to them to wash them—were agitated and left to settle. They were washed again on the 31st May and 2nd June. The deposit by reflexion appeared as brown gold. When shaken up, XXXVI gave purple or violet light by transmission, whilst XXXVII seemed too coarse for that—but LVI*a* gave a very good blue violet light, though before, it did not—this light seems to be an effect of the particles acting by diffraction.

14771. On the 4th June examined all the specimens XXXVI—settles well—aggregated into visible flocculi, which were black as to transmitted light but by reflexion of a deep brown when a black ground was behind them—put into a watch glass and the alcohol removed by imbibition and evaporation—then black by transmitted light and light brown by reflected light, because of the removal of the surrounding liquid medium—the face of the

metal in contact with the glass had a fair golden metallic reflexion. No. XXXVII was as XXVI[1], except that by transmitted light a little deep violet blue was apparent. LVI*a*, looked at generally by transmitted light, had a rosy tint; by reflexion the gold was brown. On agitating, part shook up in powder particles but a large clot adhered together—poured off the fluid and its particles—it was well violet blue by transmitted light, shewing the particle action—but the clot by shaking about cohered and became burnished against the glass, being then fine golden yellow by reflexion and opaque by transmitted light. In other points, this sample was as XXVI[1] and XXXVII.

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14772. All these specimens were alike when dried in the watch glasses, brown by reflexion. Heat on the sand bath cause no change and no signs of phosphoric acid. When heated by blow pipe to higher temperature, the gold acquires a lighter brown colour, but I could not detect the formation of any phosphoric acid at this time. I think the gold is metallic and not as a phosphuret, but am not yet quite sure.

14773. The day before yesterday, I put a comparatively strong solution of own gold over phosphorus in a large dish, taking care that there was no phosphorus particles floating to form films. Next day, it was seen that red fluid formed, but not so quickly as expected. Still it formed, when the gold was in large excess, and remained colouring the rest of the fluid when stirred up with it. Certainly this red product is the same as that produced when the gold is weak and neutral and exhausted by the phosphorus, i.e. when phosphorus is in excess. To-day decanted off about $\frac{2}{3}$rds of the liquid, putting it into a bottle and making it LVIII, and left the rest with the phosphorus to continue the action. As it is, the fluid is altogether of a fine ruby—and full of particles which because of the strong reflexion hid the bottom (14781).

14774. Took the Pellatt Gold red glass cross as a standard of depth of colour and compared it with different liquids as ruby as I had them. The *violet fluid of no particular history*—4 inches of it equalled the cross standard in depth—it was very transparent,

[1] ? XXXVI.

as much so as the glass—but rather more violet in colour. 15 cubic inches had salt added to it, was agitated and then left in a glass to settle—the salt rendered it blue violet quickly, perfectly and much. No. XLII: $1\frac{1}{2}$ inches in depth equalled the glass standard, but the tint was rather more violet and the fluid by reflected light was turbid, indicating larger particles of gold—10 cubic inch of it were mingled with salt and left. No. LII: a boiled fluid and considerably turbid by reflected light; more so than XLII. 2 inches of it equalled Cross in depth of tint and was the nearest to it of any—it was mouldy and therefore was filtered—which took out only the mouldiness; 10 cubic inch of it and salt were mingled together. This fluid was changed in appearance by the salt but less than the two former. All was left to settle at 12 o'clk. (14783).

14775. The mucus from LII was gathered out by the end of a wood splinter and put upon paper; it was deeply coloured by gold particles—violet—it smelt of phosphorus and I found a little piece of phosphorus in one place which being heated burnt with flame. When dry, the mucus filaments appeared as dull golden matter.

14776. The cell (14760, 7). As before, parts keep their places, but the salt has crept up and now there is very little tint in the fluid in the top half.

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14777. The blue or violet deposit, LIII, washed again and put into a watch glass to settle—the fluid from over it neither acid nor alkaline, but a little salt there, chlo. sodium—washed, drained, dried—transmitted fine purple light through many particles—reflection, dull metallic as indigo—heated in the dark, no smell—no light—became as brown gold—higher heat of blow pipe did not give the usual lighting up, and the gold was left so as not to burnish by glass rod but was brittle and gritty, as if something besides gold was there (perhaps tin). Threw the rest of LIII away as being an unsatisfactory preparation.

14778. A portion of XXXV (14765) had lime water added—by standing, a precipitate of like colour formed—poured off the supernatent clear fluid—added a little M.A. which dissolved the body of the flocculi, i.e. the lime, leaving the gold purple and very small in colour appearance.

14779. De la Rue solution of Gold—12 drops evaporated in capsule and decomposed by heat left 0·85 of a grain of metallic gold.

14780. Six drops of the same solution were put into 100 cubic inches of water and placed over phosphorus in a clean dish at $5^h\ 45'$. On the morng. of Friday, the 6th, there were very poor signs of action—little violet here and there about the phosphorus but nothing like what it should be—left to go on. Monday the 9th, very poor action—has been too weak—phosphorus is clean, not dark or covered—dismissed it altogether.

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14781. On Wednesday the 4th I put some strong solution (by comparison) of gold over phosphorus—found that the liquid (14773) LVIII over the gold had strengthened, so poured it off and put part of the remainder over to carry on the process (14786).

14782. As the Six drops were too little, put 10 drops of de la Rue solution into 70 c.i. of water. Made a fresh clean solution of phosphorus in sulphuret of carbon—deposited the phosphorus by evaporation in a small glass basin, leaving it some time in the air to evaporate, and then put all on at 10 o'clk. In a few hours seemed to promise well (14787) LX.

14783. Yesterday resumed the three precipitates of (14774), O, XLII and LII; the clear fluid was poured off the deposits, which clung well to the glasses, fresh water was added and each was shaken well up to wash and then left until to-day. To-day each was treated thus—the clear fluid containing no gold was poured away—4 drops of Hydrochloric acid and 4 drops of Nitric acid were added to each glass and a little warmth applied to dissolve the gold; this was effected perfectly. The solutions were transferred to watch glass, each glass being washed with a little water and the solution added to the rest and then the respective solutions were evaporated to dryness and heated, so as to leave the metallic gold in the capsule as a large spot about the size of a sovereign. The gold was in too small a quantity to weigh, and so a set of comparitive glasses made thus. One drop of De la Rue solution was put into a capsule and treated in the same manner and its gold obtained. Another drop was diluted with water and a fourth

of it taken and treated in like manner. Two fourths of the same portion was treated in the same way. Also a full drop was added to the remaining portion and treated in the same way. Hence a series of capsules with gold, and I found that ocular examination enabled me to place them with some confidence in a certain order.

14784. The preparation XXX A was in a flask and consisted of the deposit of XXV, boiled, and distilled water (14549, 50, 615, 724, 39)—the fluid is violet and turbid—neither acid nor alkaline, and the chlorides in it are not sensible to nit. Silver—it was poured off the sediment which was fine violet by transmitted light, black in the middle and brown by reflected light—the deposit was transferred to a watch glass; the liquor returned to the flask was still marked XXX A. The deposit being dried left the gold as a copper brown metallic film (with glass particles from stirrer); by heat it brightened in tint and then as to quantity of gold seemed to come between capsules LII and XLII (14783). I dissolved it in the capsule by N.M.Acid, evaporated it to dryness and reduced it by heat of spirit lamp, and then the colour of the gold became as in the other capsules and in appearance it seemed to come about or after that of the *one drop*. No acid, phosphoric or other, was evolved when the gold was heated—there was no appearance of phosphorus in it. The fluid XXX A being evaporated in a capsule left the merest trace—neither acid nor alkaline.

14785. Again examined the gold in these capsules and believe they stand in the following order:

12 drops De la Rue	0·85 of grain of Gold.
{ $1\frac{1}{4}$ drop	0·0875
{ LII very like the former	0·0875 in 10 c.i.
{ 1 drop—not much different to XLII .	0·07
{ XLII—very like the 1 drop	0·07 in 10 c.i.
XXX A—between XLII and the $\frac{1}{2}$ drop	0·052 in the deposit
$\frac{1}{2}$ drop next	0·035
O—a good deal less than the $\frac{1}{2}$ drop .	0·02 in the 10 c.i.
$\frac{1}{4}$ drop—nearly as O, but a little under .	0·018

So the quantity of gold in the coloured fluids and in the deposit[s] is very small: the quantity is not inversely proportional to the depths of fluid required to produce different depths of tint,

for the lengths for O, XLII and LII were 4 inches, $1\frac{1}{2}$ inches and 2 inches, but it is known that the tint depth is not in proportion to the gold merely but depends upon its condition of divisibility. As 10 cubic inches of water weigh 2524·58 grains, so there is only $\frac{1}{126,230}$ by weight of divided gold in the O fluid to give it the tint it has—and $\frac{1}{36,065}$ by weight of gold in the fluid XLII. As a leaf of gold weighs 0·1893 of a grain, so it could colour 6827 grains of water up to degree of fluid XLII; this quantity is equal to 27 cubic inches of water, and that is equal to a square prism of 10 square inches base and 2·7 inches high—hence such a quantity of the fluid contains enough gold to make a leaf of ordinary thickness and size.

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14786. No. LVIII (14781). On the 9th, poured the liquid from over the phosphorus and put the other portion on. On the 11th, poured the latter off also and put both liquids together into bottles as LVIII; they are very turbid. Some gold had deposited on the phosphorus.

14787. The solution (14782) was put into the basin on the 6th June. On the 7th, the ruby colour was coming on. On the 8th, it was good. On the 9th, better—rather purplish. I have a glass plate of this shape, coloured on the four parts *gggg* by gold of a ruby tint—which though not the same tint as that of the solution can be compared in intensity with it. Now a depth of liquid of 2·7 inches appeared equal to the gold tint of the cross. On the 11th, the colour was increased and a depth of 1·3 inches equalled the standard. On the 12th, the effect did not seem much changed and a depth of 1·4 inches equalled the standard. I then poured it off from the phosphorus, but there may be gold in solution still. Being left standing until the 16th, it had I think become more turbid and some gold powder had settled. I poured of[f] the fluid portion from the very bottom part—put both into bottles and made them LX*a* and LX*b*.

14788. Ten drops of De la Rue's solution contain 0·7 gr. of gold (14785) and this is the utmost that can be present in the 70 c.i. of water (14782), which is just 0·01 of a grain in a cubic

inch of fluid—but I must examine whether all the gold present is reduced. ($\frac{1}{25250}$ of gold[1].)

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14789. On the 13th instant, I put some ruby fluid with salt which immediately rendered it blue, and I left it. It soon settled, but being left until the 24th, the supernatent fluid was of a pale yellow colour as if part of the gold had dissolved again—the blue deposit had disappeared; the fluid (still undisturbed) examined by Sun light and a lens shewed no diffused particles—it was acid to litmus—and by protomuriate of tin it gave a buff precipitate, which on the addition of pure M.A. darkened, diminished and became Purple of Cassius, so that there was gold in solution (14939). There may have been some gold in solution in the original ruby fluid. The yellow fluid, being poured off, left a little heavy aggregated deposit of gold—not soluble in M.A. in 24 hours, but dissolving on the addition of a few drops of N.A.

14790. The yellow fluid (14789), placed over phosphorus, was in a few hours altered—the tint passed away and the fluid became colourless—gold film formed on the surface and gold particles deposited on and about the phosphorus below—the particles were comparitively large and there was no appearance of *ruby* formation.

14791. The yellow fluid (14789) with a little ammonia to neutralize it was rendered turbid—more ammonia cleared it up and took away the yellow colour.

14792. The yellow fluid (14789) evaporated to dryness gave much salt tinged yellow, which by a higher heat rose to a mixture of tints, rose, ruby, brown. Washed out the salt and found some sulphate of lime present, forming minute crystals. After eliminating these there was nothing extraordinary—but sulphate of lime seemed to enable gold to render it rose coloured when heated. The gold ultimately is left as brown gold by reflected light and is of a dingy violet by transmitted light.

14793. *Violet fluid, no particular history.* So labled. This fluid is now of a *fine ruby colour* and very clear—but there are two or three pieces of mouldiness within beneath, of a dark blue or black

[1] In pencil in the MS.

colour—these have gathered all the deposited particles and probably helped to gather them, so that there is *no deposit* at the bottom of the bottle. These being drawn out by a splinter of wood were transferred to water—and being shaken gave out the blue violet particles to a considerable extent, and so a dense violet fluid was produced. A sun's ray and lens still shewed particles suspended, very fine and being those which confer the present ruby colour. The fluid is very slightly acid.

14794. This feebly acid ruby fluid (14793) experimented with as to change of colour by acids, ammonia, and salt. It changed to violet blue at once by a little Hydrochloric or Nitric or Sulphuric acid—and also by a little salt. Ammonia being added until alkaline, even much, did not change back the colour, which remained blue. The sun's ray and a lens shewed the gold particles as of a brown golden colour. Heat caused flocculescence of the preparation with the salt, but agitation broke that up, and the others did not unite—the flocculi were blue whilst they continued.

14795. The same ruby fluid (14793) with enough ammonia to make it slightly alkaline *did not change colour* (10810). The same ruby fluid with little ammonia to neutralize it and then salt added to the same amount as before *did not change colour*—but after some hours a ruby precipitate formed, which may be a good process for obtaining that form of particles.

14796. The same ruby fluid (14793) with a large proportion of common salt—became Violet blue at once—then boiled together, but there was no sign of any solution of the gold or any appearance of yellow colour. Perhaps more acid should be present.

14797. Took another ruby fluid, No. XLII (14535, 614, 740, 63, 74, 84, 5). It is at present a fine ruby—turbid—deposit floating up in clouds very easily if moved. The upper clearer liquid—part poured off for experiments; this liquid with sun's ray and lens gave very fine cone, particles being very numerous but light—there is a very little excess of acid present—no mouldiness—Nitrate of silver shews chlorine, giving a precipitate that takes down the gold and is of a fine violet colour. This ruby liquid with common salt, Hydrochloric, Nitric and Sulphuric acids changes to blue—the cone of light rays shews the solid particles of gold in all. The same fluid with ammonia did not blue; with

ammonia and salt—there was a slow change, but the general action was just as in the former case of a weaker fluid. When fluid with ammonia had a little S.A. added to neutralize the alkali, the excess of acid instantly turned all blue, so that Ammonia had not fixed the ruby condition of the particles. When any thing has changed them to blue, they do not pass back to Ruby (14824).

14798. The ruby fluid (14797) boiled alone in a tube became violet blue and then deposited in flocculi. Does the excess of acid and of the solution of gold contribute to this effect? The ruby fluid with addition of a little sol. phosphorus in sulphide carbon in a tube, boiled—the gold separated in violet particles or flocculi—the fluid became colourless but the sun's rays shewed particles, distinct, moving about, and there is a little ball of clean phosphorus at the bottom.

14799. Some blue fluid from the mixed glasses of 14797 with a little phosphorus in sulphide carbon in a tube, boiled—colour all goes and the gold aggregates as in the last case—the appearances are the same—there is no phosphorus power either of keeping or returning towards the ruby condition—look[s] much as if that condition were mere disintegration.

14800. Now wrought with another fluid LVIII (14773, 81, 6) which has much deposit as well as much gold in suspension—it is strongly turbid but by transmitted light of a fine ruby colour. The fluid was drawn off carefully by a syphon from the deposit. The latter was transferred to a smaller bottle and water added to it to wash it—it was brown by reflected light and of a fine violet by transmitted light where not too dense: it was marked LVIII *b*.

14801. The fluid LVIII left (14800) was acid—by proto chlo. tin it darkened much, as if it contained gold in solution—by Nitrate of Silver there was plenty of precipitate, shewing chlorine. When filtered several times through a thick paper filter, the rose coloured gold was stopped on the paper, being caught there; and then the passing fluid, freed from most of the suspended gold, shewed the gold in solution, being of a faint yellow colour and precipitating purple of Cassius by proto chlo. tin.

14802. By boiling in a tube, the fluid LVIII left (14800) lost of

its ruby colour, became more luminous and filled with particles more sensible in size though fewer in number–it was left until the next day when it had generally a rosy tint, but much had settled into a dense little compact cloud at the bottom, which was deep blue when the strong light of the day was seen through it, but with a duller direct white light it often appeared ruby or rosy according to the accidents of reflection, refraction, etc. etc.

14803. LVIII had been preserved for several days in two bottles, and when it was now transferred from these and the bottles washed, the sides and lower part was found tinged or a rosy or ruby hue, from the adhesion of some finer particles of gold. This adhesion continued whilst the bottles were well washed out. Access of air did not alter this tint. A solution of salt, even up to brine, put into the bottle and left there an hour, did not alter it in the least. This being washed away, perfectly–and strong Nitric acid introduced, still the rosy gold was unchanged. This being removed, Muriatic acid was brought in contact with the gold and did not change it. Neither did Sulc. acid affect it. A little N.M. Acid dissolved it at once, producing the usual solution of gold. I believe that the finely divided gold constituting the ruby state, being held in place by their adhesion to the glass, could not cohere together under the influence of the brine, acid, etc. and therefore were not changed in colour. Any chemical change was quite free to take place.

14804. Filtered Ruby LVIII (14800) through a thick filter, leaving the ruby gold on the filter and colouring it. Passed a solution of salt up to brine through the filter; it caused no change of tint though a little of the same salt [illegible] a little of the fluid itself instantly altered it, destroying the ruby tint and causing the gold to appear in adhering visible particles. The brine was washed out of the filter well and then the moist and coloured filter cut up into pieces and put into test glass[es] with the following fluids about them. Strong sol. salt–Hydrochloric acid–Nitric acid–Sulphuric acid diluted–water for a standard of comparison–Sol. Ammonia–of Potassa–of Soda–of Sulphuretted hydrogen. No change occurred with any of them–thus confirming the results with the bottles (14803)–but they were left for the effect of time (14811).

14805. The salt I have been employing contains a good deal of sulphate of lime, which accounts for the appearance of small crystals of that body appearing now and then.

14806. Six drops of De la Rue's gold into about $1\frac{1}{2}$ pints of water to make a fluid of equal strength. The half of this taken, and 10 drops of pure hydrochloric acid added, so that the two solutions were of equal gold strength but one neutral and the other acid. Four glasses were prepared. Against the sides inside of two of them slices of common phosphorus were made to adhere. Against the sides of other two Phosph. from the Evaporation of the sulphide of carbon solution was made to adhere; then the Glasses were numbered and the gold solutions introduced so as to cover the phosphorus and be fit to produce the red or ruby fluids.

No. I. Was common phosphorus and neutral gold solution.
No. II. „ common phosphorus and acid gold solution.
No. III. „ sulphide phosphorus and neutral gold solution.
No. IIII. „ sulphide phosphorus and acid gold solution.

All these were then left covered over until the next day.

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14807. To-day, after 20 hours, the state of the glasses was as follows: No. I. Violet ruby at the bottom over a small extent—but little effect as yet; but still this phosphorus without sulphide produces it. Stirred up the fluid and left it. No. II. There is less effect here than in No. I, but it is peculiar and instructive. There is no colouration of the fluid, but there is the course of streams marked upon the glass up and also down from the phosphorus. In these places the gold is fixed upon the glass, and has a metallic gold reflexion, both on the glass and the solution side. The appearance by transmitted light is of blue, violet and ruby, varying with the circumstances of the light, but the ruby is clearly there: is a fine case of variety of tint and appearance of the same divided gold. Less effect than in No. I. No. III. Presents, as to the formation of ruby fluid, the finest result of all, but then the patch of phosphorus is much larger than in the other glasses and still adheres to the side near the top of the fluid. The ruby is strong at the lower half, and there is a little bright metallic film on the

top—being stirred up, all was strong ruby. It seemed very clear and free from turbidness, but the lens and sun's rays shewed the fine dissemination of particles. There is no adhering stain as in the No. II glass. No. IIII. The small patch of phosphorus, i.e. as small as No. I or II, has fallen to the bottom—there is no sensible colour in the fluid but there are violet patches about the phosphorus at the bottom of the glass. Stirred up and left. The acid evidently seems to interfere with the production of ruby, and seems to prevent diffused particles.

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14808. As to the glasses (14806, 7): No. I is getting on—the fluid is violet all through; there is a deposit on the glass round the phosphorus which is golden metallic by reflected light, yet translucent and rosy or purple or violet according to the thickness by transmitted light—a ray of light shewed particles in the fluid. No. II is progressing very slowly—but there is a little violet at the bottom, which stirs up into the liquid. The chief effect is a dark deposit on and close to the phosphorus, and the stream mark of like nature running to the bottom—all this is metallic gold by reflexion and transparent violet by transmission. Where it is thinnest, there it approaches most to ruby colour. The action is slow—there are particles in the liquid. No. III shews a fine effect—fluid is ruby and turbid—the light cone fine—there is a little film on the glass near the phosphorus on one side, in character like the II Glass film. The reduction seems nearly complete. No. IV. There is a little effect and the fluid is violet but poor—there is black portion of gold round the phosphorus at the bottom.

14809. No. V. Made a weak solution proper of my gold solution—neutralized it by a little ammonia and put it into a glass with a good large patch of phosphorus from solution in sulphide on the side. The object was to ascertain if the removal of the excess of acid by ammonia was advantageous to the reduction.

14810. The ruby fluid with Ammonia (14795) still ruby as before.

14811. The glasses with rosy paper in various fluid[s] (14804)

left on Friday, i.e. yesterday, are absolutely unchanged; neither the salt or any of the acid or the Sul. Hydrogen had power to alter any of the held particles from rosy or ruby to blue. So extended the trial by other agents. The fluid was poured off from that with Salt, and a portion of very dilute solution of chlorine poured on it at once turned the rosy tint to blue and gradually dissolved all the gold away. I think the finer ruby particles are dissolved first, leaving the blue to manifest their presence, which being larger take longer time to dissolve. However, size cannot be the only difference between ruby and deep blue gold particles. Then poured off the potash solution from its glass, washed the paper with a water, removed it and then poured on a weak solution of cyanide of potassa, and afterwards a stronger solution. There was very little action and very slow solution; and that ought to be so, the gold particles being immersed in the fluid—they did not become blue. On bringing the gold paper to the air and leaving it, the gold gradually dissolved, becoming blue before it disappeared, just as happened with the chlorine but more slowly.

14812. Took the piece which had been in water only—placed it on a glass plate—folded it so that two ruby parts laid together face to face, and then rolled and pressed a place with a glass rod, endeavouring to displace the particles mechanically and convert them by adhesion into blue particles. The rosy tint certainly disappeared and I think a blue tint came in its place. The cohesion of the particles together would render them less visible, but whether that was the effect or whether they had merely been displaced and washed away I am not sure. Washed the other five pieces of rosy paper well, and then dried those from the three acids (14819).

[A pocket made in a blank page of the MS., and bound into the volume at this point, contains several pieces of the "rosy" and one of the "blue" paper.]

14813. Ruby *fluid* with solution of Sulphuretted hydrogen (weak) became purple and then slowly blue.

14814. Ruby fluid LVIII (   ) with sol. Sul. lime—the ruby disappears and the gold is aggregated. The same ruby fluid poured on to a piece of cast plaster of Paris produces a rosy tint,

like that on the filter, but by pouring more and more on the same spot, so as to accumulate gold, a rotten surface is gradually produced, and the gold left tends to have a violet colour, yet with much ruby in it. The gold had been carried some depth into the plaster.

14815. Some of ruby fluid LVIII, with little ammonia to neutralize it, became rather more violet—added sol. of salt and left it to precipitate, which after a time it did in flocculi. Next morning, bulky settlement—poured off the fluid and added water to wash—the mixture was ruby violet—washed again and obtained a purple red precipitate, bluish when looked through. Dilute Mur. acid put on to the precipitate dissolved much, and being left to act, brought away some gold in solution and left a dark purple deposit. More mur. acid dissolved away more and left a dark body, chocolate brown by reflected light, ruby and blue by transmitted light according to the accident and quantity of the light. Strong Mur. acid on this deposit seemed to cause no change when cold, but by heat the powder became bluish and much of it dissolved, giving a solution containing gold and leaving a few heavy particles of gold. Some of this substance was put upon glass slips and, being dried, left spots of bronze purple particles, translucent and purple by transmitted light, and consisting apparently of crystals of some somewhat transparent salt. By sufficient heat the purple spot became golden by reflection and rosy by transmission. Agate pressure on the heated or unheated [part] gave bright reflection and green transmission.

14816. I think a phosphate of lime must have been formed here and taken down the gold with it. Phosphoric acid in the ruby fluid—Sulphate of lime in the salt—Ammonia added, etc., all tends to this conclusion, or to formation of phos. amm. and lime with gold (14832).

14817. The Ruby fluid XLII (14796, 7)—with common salt and Mur. acid, boiled together; the gold dissolved—the colour disapprd. and the proto mur. tin shewed gold in solution. Even gold leaf will dissolve on Mur. acid pure by the aid of the air action (see 14939).

14818. Ruby fluid with little of De la Rue's solution of gold added—the tint did not change in several hours.

14819. Rubbed face of ruby paper (14812) with glass rod longitudinally—did not succeed in producing a blue tint.

14820. Ruby paper (14812) moistened by different media to notice optical effect of colour. Moistened *with water*, the ruby tint darkened a little by the transparency, etc.; drying restored first tint. *Ruby paper* moistened by *camphine*—darkened much more—seemed really as if blued somewhat, but upon drying off the camphine the ruby tint returned—boiling in the camphine made no change in this respect, no final change. *Ruby paper* moistnd. by *absolute alcohol*. Effect intermediate between that of water and camphine: heat applied—no final change. *Ruby paper* moistened by Sulphuret of carbon—made it look very blue, yet when dried off, the original tint returned no change by boiling. It was good to see the effect when one end was wetted by water or alcohol and the other by Sulphuret of carbon—to mark the difference and to see how by drying both returned to the first tint.

14821. Ruby paper and Almond oil—apparent change more than alcohol, less than camphine—by heat no change though temperature raised until the oil became a little brown and the paper charred. Removed the oil by camphine and that by Alcohol and dried the paper—it was charred—but the tint of the gold was not chemically or otherwise altered.

14822. Ruby paper and solution of caustic potassa—cold, no change—boiling heat, cause change to blue black—which paper and potash alone in another tube did not do. Poured off the alkali—washed—added dil. S.A.—no return; added N.A. to the S.A., rather strong—still no return; boiled and then the dark tint gradually passed away and something like the original appeared—so that when washed and dried, the rose tint was much restored but still rather more violet than before. Again added potash and the bluishness returned, but not so much as before though heat was given. Water washing alone now restored the rose tint in part, and adding a little Nitric acid restored it considerably. Finally, the addition of a little M.A. to the N.A. dissolved all the gold and rendered the paper white.

14823. Ruby paper and N.A. cold—tint not altered, except a little by the wetting. A boiling heat broke up the paper into pulp and the colour disappeared. Ruby paper and strong Mur.

acid—whilst cold perhaps a little blued—heating gradually, the gold blues a little and dissolves, and at last the paper broke up into filaments (14833).

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14824. Took [some] of the Ruby fluid XLII (14797) on the 28th June and treated it as LVIII had been treated (14815); i.e. ammonia added, produced no change—then plenty of salt added, colour became more violet; being left, a bulky precipitate formed—poured off the supernatant liquid and added water to wash—this caused much suspension of the particles and the fluid seemed uniform and violet. On Monday the 30th June, it had settled and there was a precipitate like that of LVIII, but finer in colour and I think with less brine or salt of lime in it—poured off—washed and left to settle. To-day, July 1, it is settling, of a dark purple colour—the difference to the former precipitate of LVIII seeming to depend on the proportion of phos. lime present—poured off the fluid—added strong M.A. to dissolve out phosphate—great diminution and only a little fine deep blue gold left.

14825. Continuation of the glasses (14808). No. I is now the darkest and finest in colour—generally violet but ruby rather more at the bottom—very clear—yet the sun's rays and a lens gives a fine yellow cone—no turbidness to common sight except in the sun light—there are light and settling clouds to be seen then—it presents a good action of common phosphorus. No. II is progressing, but its character just as before—general tint very weak—but gold adhering to the glass. No. III is turbid in all lights—opaque nearly in sun light—by transmitted light it is of a fine ruby colour—the phosphorus is clean. I think the gold is nearly all out of solution and now gradually aggregating. No. IIII is as poor as No. II (14844).

14826. No. V (14809) has now a deep blue black sediment—the fluid has also a dark tint and by lens and sun light shews diffused gold. There is no gold left in the solution and the liquid is neutral. The gold settles easily and is the *blackest I have yet had.* Washed and set it aside (14844).

14827. The ruby fluid XLII (14797). One specimen in a tube as a standard, marked i. A second specimen boiled (14798) and

marked ii—a third specimen had a little ammonia added to neutralize all acid and was then boiled and marked iii—this was on Saturday, the 28th June, and all were alike when left at 5 o'clk. P.M. On Monday the 30th, i and ii were alike in rosy tint, but ii shews striæ or clouds of settling, which i did not, being uniform; iii was rather more rosy than the others and shewed no settling. So far the action of boiling and ammonia is clear. Then as to action of salt on the original and the neutralized fluid: added a drop of *pure* solution of salt to i and also to iii, ii becoming now a standard. Tuesday, 1 July—i, no signs of ruby; there was a faint blue in liquid but nearly invisible, and what had settled was so contracted it could scarcely be seen—the salt had separated and changed all the gold in this original portion. On the other hand, iii had no settling and was of a full rosy colour—it was apparently unchanged—so much for ammonia effect; added much salt—at 9 o'clk., it produced a little purpling—and at 11 o'clk. there was no more (14842). No. ii was as before—had striæ of settling and was a little purple compared with iii.

14828. On Saturday, the 28th June, prepared five specimens of ruby fluid No. XLII in five test glasses. No. 1 was alone for a standard. No. 2 had a drop of pure solution of salt added to it, which turned it blue at once. No. 3 had a little ammonia added—no change, was still rose. No. 4 like 3, but with drop of pure solution of salt—very little immediate change. No. 5 had ammonia like 3, and then a little of the salt containing sulphate of lime was added—after a while it took a lilac tint. Tuesday, i.e. to-day. No. 1, rose as at first. No. 2 has settled, deposit blue. No. 3, rose as No. 1—no deposit. No. 4 is blue and settling. No. 5 has a more bulky precipitate and it is of a lilac colour—like the former combination of LVIII and XLII with the lime and phosphoric acid (14816, 7).

14829. Yesterday, a weak solution of De la Rue's neutral solution of proper strength to give ruby fluid was put into a bottle—a little solution of phosphorus in sulphide of carbon added—and the whole well shaken together; it began to change at once, and was made No. LXI. In an hour it was of a good purple ruby colour, with some dark deposit (very little) as of gold about the particles of phosphorus and at the bottom—in four hours

more it was of a fine ruby colour, and to-day it is very fine (14843).

14830. Yesterday, a like solution of De la Rue's gold and a few drops of sulphide of carbon *without phosphorus* were put in like manner into a bottle and shaken—there was no immediate change. It was made LXII. To-day the fluid has a ruby colour and [in] sunlight is turbid and shews the cone of illumination. So that phosphorus is not essential to the reduction of the gold—and former experiments shew that the sulphide of carbon is not essential (14843).

14831. Yesterday afternoon, a like *acid* solution of gold and sol. of phosphorus in sulphide of carbon were put together and shaken and made LXIII. To-day it has a dark purple colour—the acid shewing its presence and action—examined by sun light, the fine particles and also coarser ones were made manifest (14843).

14832. I have cleared some salt from its lime by carb. soda—boiling and neutralization of alkali by M.A.—it is now free from lime, and will in that respect call it *pure salt*. Ruby LVIII (14800) with plenty of this salt solution added gradually lost its rose tint and became a blue so feeble as to be almost colourless, but the sunbeam and lens shewed the diffused particles both in the rose and blue parts. So pure salt changes the colour.

14833. Ruby paper (14812, 20) was heated in dry vapour of alcohol and ether until the paper charred—but there was no bluing of the gold—no apparent change of the tint before further effect was hidden by browning of the paper.

14834. Vibration of light. Relation of the length of lateral excursion to the length or extent of axial excursion—and relations of both to the dimensions of the fine particles of gold.

14835. *Leyden battery explosions.* Exploded gold wire by the large machine and great Leyden battery as before (   ). Placing *under* the wire and *above* it—Mica—crown glass—topaz—rock crystal—plates polished—fluor spar, a cloven plate—and white paper. The deposition of gold was the same in character with all: a line abraded, which was covered more thickly with gold, and was ruby in colour, and green and blue side parts shading off into insensible quantities of film. There were differences of abrasion: thus Mica was more abraded than the glass—the

glass more than the topaz—the rock crystal least. This makes a little difference in the hold over the gold, and so the shade is caught differently; in the glass much is driven in, whilst on the rock crystal it is seen in small globules, which driving right and left, have swept the previously deposited film of finer particles before them and so left clear lines with the globules at the end. Paper, which catches the finer particles and holds them better than the other bodies, often shews ruby tints at parts an inch or more distant from the middle heated line. At that middle line I think the ruby tint is produced by the (after) heat, which has affected the particles there. See Results Nos. 386 to 405.

14836. Made the explosion of a like gold wire over the same substances in the same manner in an *atmosphere of oxygen.* The effects were precisely the same, in colours, disposition, etc. etc.

14837. Made the explosion of a like gold wire over the same substances in the same manner in an *atmosphere of hydrogen.* The effects were precisely the same as in the air and in oxygen.

14838. So the condition and character of the divided gold is in no way affected by the atmospheres or of the substances thus employed.

14839. The gold deposit wipes off freely from all the substances except where it is abraided.

14840. Heat applied to the deposits of divided gold on the above substances has exactly the same effect on all of them—the deposits on Glass, Rock crystal, Mica, topaz, are all affected alike. The general effect is to convert the more distant and finer deposit into ruby from bluish grey or green as they might be. Many green parts near the axial line are not altered from green but remain so. The heated gold, whether rubified or remaining green, wipes up from the glass except where the latter is abraided, and it wipes or rubs up from the Rock crystal and Mica, and from the topaz in the ruby part, but it adheres to its smooth surface at the parts nearer the axis of discharge. Neither substance of the plate or nature of the atmosphere used for the deflagration has any influence over the results. The finer parts of the film rubify best by heat.

14841. Agate pressure converts the ruby by heat into a beautiful green upon the Glass—Rock crystal—topaz and Mica (the last

having glass beneath it) alike—the effect optically is very striking. So no exception from the general results. The pressure effect on the central ruby on Agate was very excellent. The same effect of greening is produced on the parts not yet heated which would rubify by heat—and especially on the Agate.

2 JULY 1856.

14842. To-day, the No. iii (14827) had settled—clear violet deposit—dismissed all.

14843. To-day, No. LXI (14829) is as yesterday—ruby—very fine particles—fluid is clear to the eye by common light. No. LXII (14830)—turbid both by common and sun light—reflected colour brown—transmitted, fine violet. No. LXIII (14831), the fluid is deep violet blue—black deposit about the phosphorus particles below. It supplies a case of very fine violet blue fluid of suspended particles, and they must be very small. Think there must be some physical difference between ruby and blue besides size of particles.

4 JULY 1856.

14844. Continuation of glasses (14808, 25). No. I is now turbid, cloudy and ruby, as if the larger violet particles were aggregating. As a blue or violet fluid settles, it leaves generally a clearer *ruby* fluid containing the smaller particles. No. II fluid is of feeble colour; the gold adheres to the glass. No. III is turbid, more ruby than I, i.e. less violet. No. IV fluid is very poor in colour; the gold has been deposited at the bottom. Portions of all these fluids were tested by proto chloride of tin for gold in solution—none appeared. Nos. I and III changed in tint to purple but that was to be expected.

14845. No. LI is the purple fluid, boiled on the 11th April and left in a beaker since then (14679, 80, 705, 40). The liquid is clear and poured off without any sediment; the latter adhered well to the sides and especially the bottom of the beaker in which it had been kept—washed. Now whilst wet, this deposit was of a pure violet colour by transmitted light, but as it dried, first on the sides and then on the bottom, it changed to a fine green. Also, whilst wet the light reflected from the bottom layer was dark and

purplish, but on drying it became metallic and golden and a good golden surface, as gold leaf [illegible]—*the green transmission and gold reflexion* appearing at the same time.

14846. Tried to re-wet the film and restore the former violet and unmetallic state—but wetting did not produce the effect—the gold seemed now to be in lateral association or continuity, and was in fact as *if it had been agate pressed*—so when wetted, the metallic reflexion and the green tint remained—it was evidently adhering well to the glass whilst before the fluid was poured off for washing did not remove it and when washed and dried I think the gold would cleave still closer to the glass and be as it were card pressed—the result seems to be in some degree of that nature.

14847. Tried other fluids of high refractive power on the dry parts. Alcohol—Ether—Sulphide of carbon—but they did nothing, i.e. did not reverse the change. Strong solution of ammonia did nothing—nor solution of soda to any extent—but a strong solution of caustic potassa did reverse the tint very considerably. Strong N.A. and dilute Sul. acid were put on in drops and did nothing but loosen the gold a little. Now washed off these different drops and the potassa, and the effect at the latter place disappeared, but at the same time *a fine ruby place* came out along a line which had probably been the edge of some former trial fluid. Washed with water and this gradually reduced the ruby tint to grey nearly, and the other parts of the film gradually took on grey—it also began to break up and come off in fine thin adhering flakes. When washed and dry, poured on caustic potassa again—but the return to violet was not as before; the gold probably had aggregated somewhat. Washed the gold and let *part* dry, and that as it dried changed from its wet blue grey tint to a green, but not to such an extent as at first (14846). Strong N.A. added to both dry and damp parts: the blue remained blue—not changed to ruby or purple; applied a little heat—this impoverished the blue and made it grey. Acid removed—film washed—potash put on—still no ruby or violet. Was blue grey, thinning in parts. Alkali removed—washed—strong Mur. acid on—did not change the film—they kept their gold reflexion—and nearly all had now become loosened and was slowly dissolving. When looked through,

the films that had borne these repeated or successive treatments were grey, like De la Rue's phosphorus films on glass. Being washed and dried, there was no further change—*dismissed.*

14848. A solution of cyanide of potassium was arranged in a dish, and also a Galvanometer provided with platinum wire terminals; sometimes the platinum wire was in the solution alone and sometimes touched gold leaf either *in* or *on* the solution. One end was named A and the other B. When A and B were both merely platinum dipping in the solution, there was no sensible action on the galvanometer which I employed. When A was gold beneath the surface, and B platinum beneath, *A was as zinc.* When A was gold beneath the surface and B gold (leaf) *on* the surface—A was as zinc, though B was dissolving rapidly. When A was platinum wire *in* and B gold *on* the surface, B was as zinc. So though the gold *on* the surface dissolves very rapidly, it is not (as regards the Galvanometer) more effectual or more like Zinc—the action by which it dissolves *is quite local.*

14849. Probably compression is effective simply in altering the relation of the two dimensions along and across the ray of light, rendering a particle of this shape  for instance, more of this shape ; but then such particles must consist of many atoms, although the particles are so small. If so, it might intimate that the transmission of a given ray depends *jointly* upon the inner and outer action, as regards the particle, and not upon mere thickness of film; for thicker and thinner are still both green—and different depths of ruby are all fluid.

14850. It does not seem likely that compression can affect a ray in its length dimension, which is so much greater than the dimension of the affecting particle; but it may by its lateral extent, which is probably exceedingly small and so has a nearer proportion to the size of the particle.

5 JULY 1856.

14851. Either extra deep, deep or even paler gold, if green by transmitted light, upon examination by a lens presents fine vermicular lines or spots here and there which appear to be ruby; they do not appear in blue or purple leaf, i.e. such as contains silver enough to transmit blue. These red places might appear so

by contrast with the bounding green—the colour being subjective rather than objective; nevertheless, they are very distinct from the minute holes which, transmitting light, transmit it white. Being examined under the microscope ( ), we thought we saw a distinct transmission of ruby light through the particles occupying the place. I was led to think they were produced by bru[i]sing from the presence of fine filaments for they are almost always as lines curved or twisted; but I could not by rubbing or pressing hairs between Gold leaf and glass, paper, card, etc. produce any effect of the kind; where the gold was broken or bruised, the places transmitted white light. I am doubtful about their nature.

9 AUGUST 1856.

Observations of the numeral numbers.

14852. XX (14739) tube–violet deposit–the fluid has a faint tint of the same, with clouds, as if would entirely clear. This tube is noted as red at 14442–14321, 22–as if it had changed in colour by the 6 months since Feby. 6–but I did not shake it up to see if it would then constitute a red or a violet fluid (14880).

14853. XXI (14739) tube. Blue violet deposit–faint ruby tint in the fluid. This specimen seems to have been more ruby originally: see 14343, 443, 4, 618, 724.

14854. XXIII (14739) flask. Blue violet deposit with and amongst mucus–the fluid has little or no colour. Is as before: 14447, 544, 79, 724.

14855. XXIV (14724) tube. There is no deposit in this tube and the fluid is of a faint yellow, as if a solution of gold had been formed. There has been resolution here, 14641, 724. Would a drop of phos. in sulphuret of carbon and agitation bring on the ruby again? (14881).

14856. XXV (14739) tube–deposit violet ruby–fluid yellow as if it contained a solution of gold. This is the tube that had a green reflexion (14618, 35, 41). Must look at it in sun light to see if it is there still (14882).

14857. XXV D (14784) tube. Deposit ruby–fluid above almost colourless–seems to be unchanged from former state: 14617, 724, 39.

14858. XXV L (14739) tube. Little ruby deposit–fluid has a faint brown red colour–with cloudy parts of less colour as if the [ ] would settle. Else there appears to be no change (14615, 724, 39).

14859. XXVII (14739). Ruby deposit–fluid faint ruby. Same colour and characters as it had six months ago (14321, 445, 548, 611, 53[1], 724, 14739, 859).

14860. XXVIII (14739). De la Rue's original fluid, violet then. Now it has a blue violet deposit and the fluid is of the same colour. See 14447, 548, 615, 53[1], 739.

[1] ? 14635.

14861. XXX A (14784). In a flask—the deposit is ruby, and the fluid is of a faint ruby uniform colour. By a former note (14739), this fluid or mixture would seem to have changed from ruby towards violet: has it now changed back again, or is it only the effect of congregation (14883) of the particles? See 14549, 50, 615, 724, 39, 84.

14862. XXX B (14739). Supernatent liquor of the XXX A. It now presents a beautiful ruby deposit—the fluid also has a fine ruby colour, but is still settling and about $\frac{1}{4}$ of an inch from the top is nearly colourless. This seems to me a very pure ruby preparation: whether due to its having been poured off from a deposit and so consisting of the finer particles unmixed nearly with heavier ones, I cannot say, 14549, 615, 724, 39.

14863. XXXII (14739) tube. Prep. with Sulrt. carbon and phosphorus. It is just as on former occasions; the water is clear, so is the sulphuret of carbon below; but the blue deposit surrounds the latter (14457, 739).

14865[1]. XXXIII (14740). Violet or black deposit—fluid colourless—particles large; is as before, 14463, 550, 740.

14866. XXXV (14778). There is a ruby deposit now—the fluid also is ruby and uniform. Is as before, 14761, 4, 5, 78.

14867. XLII (14828). Ruby fluid made 12 March. Has now a ruby deposit on the side at the bottom nearest the wall—the fluid also is of a fine ruby colour. No change in colour here.

14868. XLIV (14740). Ruby fluid of 22 March, 14576, 614, 740. Now has much blue violet deposit—and mucus also—fluid is of a warm violet tint—there are clearing clouds at the top. Would this violet deposit stir up into the deep ruby fluids of 14576, 614?

14869. LII (14785). There is a violet deposit—whilst the fluid has a ruby tint—mucus is present—14697, 706, 59, 74, 84, 5, 869.

14870. LIV (14740) tube—is De la Rue's violet fluid boiled, 14740. There is now a violet deposit—the fluid has very little violet tint, so the boiling caused separation without altering colour. If stirred up, what kind of violet fluid would it make? (14884).

14871. LV*a* (14769). There is a fine ruby deposit—and the supernatent fluid is also of a fine ruby colour.

14872. LVIII. There is a good ruby violet deposit at the bottom

[1] 14864 is omitted in the MS.

on one side—the fluid also is ruby. There has been no other change here: see 14773, 81, 6.

14873. LVIII *b*, deposit (14800). This deposit is by transmitted light violet—but by reflected light golden brown—the fluid washing it is a pale violet. The washings from this deposit marked LVIII *b* in a glass have a ruby rather than a violet colour, and the fluid is ruby, as if the finer ruby particles could be in some degree separated by washing (14800). There is some mucus in the glass and it is violet blue by the gold it has caught up.

14874. LX *a* is the fluid from over a deposit (14782, 7). It has now a plentiful deposit at the bottom on one side—the deposit is violet in mass—the fluid above is well ruby. The deposit LX *b*, is a violet deposit—the fluid being clear and colourless—the deposit easily shakes up into a violet mixture. There is blue mucus in the fluid.

14875. LXI. This is a ruby solution made with Phosphorus in Sulrt. carbon shaken together (14829, 43). It is now in fine condition for ruby colour—there is a film on the top, perhaps phosphorus or sulrt. carbon—and a little ruby sediment.

14876. LXII. This was sol. gold and sulrt. carbon without phosphorus (14830, 43). To-day there is a violet deposit and the fluid has violet colour and there are suspended particles.

14877. LXIII. This was acid gold solution with phosphorus in sulphide of carbon in a bottle (14831, 43). The fluid was left a deep violet blue; now it is colourless and apparently contains no particles, but there is the deep blue deposit. So the presence of the acid works thus to cause blue particles and their separation.

14878. LXIV. Violet fluid, no particular history. Now has a violet deposit at the bottom on one side and the fluid is violet. Mucus is present.

14879. LXV. Not labled before. Ruby deposit—fluid also ruby.

14880. XX (14852). Will not shake up—the deposit clings to the glass—and adheres together so as to come off in flocculi—where it adheres to the glass it is of a blue violet colour by transmitted light.

14881. XXIV (14855). Added a drop or two of sol. of phosphorus in sulphide of carbon and agitated; the whole fluid became immediately turbid and violet, and on settling the particles of

sulphide of carbon sank to the bottom, of a dark purple or violet colour, carrying the gold which had been reduced with them, as was expected.

14882. XXV (14856). The clear yellow fluid above gave by lens and cone of sun-light the green reflexion—the fluid, of a chlorine colour, was poured off and shewed particles and a green cone. The deposit being shaken up did not adhere together, but diffused through the liquid and now by the cone of light there was no more green but a golden yellow reflexion.

14883. XXX A (14861). The deposit here has a fine violet ruby colour—it does not adhere to the glass or together, but by agitation is diffused and the whole fluid becomes a violet ruby by transmitted light. I do not think the sample has changed back—it is a fine specimen.

14884. LIV (14870) tube. The cone of light still shews particles in the fluid above the deposit. On agitating the deposit, the particles were diffused, and the fluid produced had I think the same colour and depth of tint as before boiling.

15 AUG. 1856.

14885. Nos. 235, 237, 239, being deflagrations of gold (sovereigns) by the Voltaic battery, were heated to redness. The slate violets became immediately converted into ruby, and more especially at the outer part of each deposit where there was less gold and apparently in finer particles—there the tint was most beautiful, passing into ruby violet as one came to thicker parts of the deposit and so on to pure violet—as if accumulation of the ruby would produce violet—the reflection at these places was yellow and golden. A very slight deposit of gold, hardly sensible before the application of heat, gave the ruby tint. Pressure of agate produced its effect and in some cases pretty well, but the tint produced was more purple than green—the change was very manifest.

21 AUGUST 1856.

14886. Several days ago I put a little of the ruby LXIV into a plate, to oxidize if it would and form a *solution* of gold, covering it with a glass Jar. By to-day it has become violet, perhaps from matters on the plate—but the gold does not seem to have dissolved.

Proto chlo. tin shews no gold in the fluid in solution. There is some violet deposit on the plate.

14887. At the same time put out LXI, using plenty of fluid, $\frac{1}{4}$ to $\frac{1}{2}$ inch deep—it was well ruby. No deposit here to-day, nor any sensible solution of the gold.

14888. At the same time put out XLII in like manner, it being a ruby fluid. There is no solution of the gold, but there is a rosy deposit on the plate—violet in parts; poured off the fluid; as the rosy parts became denuded of water which ran off here and there, they became violet. I could not wet them again with water to see if the tint would return; there was evidently a reflecting surface between the film and the water. Used alcohol: the parts already dry and violet remained violet, but many were wetted by the alcohol from the watery state and then remained ruby or rosy—poured off the alcohol and let the parts dry—they became bluish in the air, but being wetted by alcohol again became rosy—solution of common salt did not then affect these parts.

14889. In order to evaporate some specimens of Ruby fluid very quietly, some lumps of Chloride calcium were put into a clean pint bottle and then portions of the ruby fluid LXI were put into five well washed glass tube[s], and these into the bottle and all closed up and left. Great care was taken to wash the tubes first with distilled water and then with a portion of the solution LXI, so as to prevent anything remaining which could change the ruby to blue—but it must be remembered that all the chlorine, acid, etc. of the solution remains in the specimens. The tube marked I had a depth of fluid equal about 0·75 of an inch. No. II had less. Nos. III and IIII still less and about alike, and No. IIIII least, being about 0·25 of an inch deep. All were plentiful enough to give a sensible and even good ruby colour. Made them LXVI.

14890. Quantity of Gold in a given ruby solution. Dissolved four leaves of gold in N.M.Acid—evaporated and heated over a steam bath until crystallized—dissolved in water—filtered the fluid; there was some undissolved residue to which a drop of N.M.Acid was added—it dissolved, giving gold solution—was evaporated—dissolved and filtered and added to the former, with water enough to make 80 cubic inches—the solution sensibly neutral. Put it into a very clean glass bottle—added 4 or 5 drops

of solution of phosphorus in sulphide of carbon and agitated well–it soon began to change and was made No. LXVII–left to change.

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14891. The above ruby LXVII tried against the standard glass. About $\frac{7}{8}$ or $\frac{8}{8}$ of an inch in depth appeared to be the equivalent, but the tint was not quite the same with glass–not so scarlet or bright. The fluid seems very transparent but a candle and lens shewed the cone of floating particles. On Monday the 25th, i.e. 4 days after the formation, the fluid had the same intensity of colour and being examined by proto chloride of tin, there was no sign of gold in solution. I think the tint is more purple than it was before. As 80 c.i. make a prism of 10 square inches eight inches in height, so there is a leaf of gold in every 2 inches–and two inches in depth give 2 equivalents of colour equal to the glass standard, for each gold leaf.

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14892. LVIII gold preparations consist of three bottles made from rather a strong solution of gold in a basin over phosphorus (14773). LVIII is ruby fluid with a deposit formed since 11 June (14786). LVIII *b* is deposit in a bottle which has been washed in two waters (14800, 73) and there is a bottle of no consequence labled LVIII *washing*–containing the first washing waters from LVIII *b*.

14893. LVIII *b*–the deposit–examined first: below is the deposit, over it the washing fluid. Looked *at*, the deposit is brown, the fluid a pale ruby violet, and by the sun and lens suspended particles are found in it looking quite green, probably by contrast with the amethystine light of the general fluid. When the deposit was looked *through*, flocculi of sensible sized particles were at the bottom, and over them a *dense violet* layer of particles flowing as an adhesive heavy fluid.

14894. Syphoned off the supernatent fluid–when tested by nitrate of silver there was very slight trace of precipitate of chlorine. The gold particles carried over made the fluid in a glass tube appear brown yellowish when looked at with a *black* back

ground—the reflexion effect being then alone and so pure; but when the back ground was white foolscap so that some *transmitted* light could pass and mingle with the *reflected* light, the colour was amethyst—this is an important effect and must be carefully distinguished. There was so little gold in suspension that in common day light the fluid seemed hardly hazy, but in sun light it became as a fog from the quantity of light reflected. Put a little of the deposit LVIII*b* to it, and then the effects just described were very strong, and the sun light cast on to it reflected a brown light but that which passed through was purple or violet.

14895. The residue LVIII*b* ( ) was so thick that when stirred up with the fluid about it, it made a sort of paint, which running over the glass in a thin film, was brown by reflected light but a fine blue by transmitted light, with little or no signs of ruby or red seen in this way. A drop of this mud into pure water made it a very interesting fluid. It often looked violet and ruby in general lights, but by reflected light only, using a black back ground, it was a pale reddish brown; and by transmitted light only, it was a blue tending to violet, but always of that tint, from a film hardly coloured to a depth so great that scarcely sun light could pass through it; but by mixed lights was often ruby violet though dull in tint. Put the third water on to this gold and left it.

14896. LVIII, i.e. the fluid preparation referred to above (14892), is of a beautiful ruby colour—the deposit at the bottom of the bottle, when looked at from below before disturbance, seems also ruby at the thinned edges, but the light which comes through it came also through the ruby fluid above. Syphoned off the ruby fluid from the deposit and dealt with the latter first, making it LVIII*c*. It presented exactly the same phenomena as the former deposit LVIII*b*, and was left with pure water over it. Hence both these deposits from fluid LVIII are blue by transmitted light.

14897. The mode of examining these deposits and the fluids by transmitted light alone was as follows. A glass tube closed at the bottom was surrounded by black paper so as to shut out all side light, then more or less of the fluid was poured into it and a sheet of white paper lying on the ground and well illuminated was looked at as the source of light; when so much fluid was

there that little of this light passed, then holding the tube up and looking to the sky or a sun cloud or the Sun itself gave stronger light; in this way very faint tints or very deep tints could be examined and the effect of little or much fluid observed.

14898. Little or much depth of fluid appeared to make *no change* in the tint that passed—there was no transition from one tint to another when side lights were excluded, and so no effect of colour due to more or less absorption of the transmitted light.

14898½. LVIII fluid—with nitrate of silver shewed the chlorine present—with proto chlo. tin shewed gold in solution. When examined in tube by transmitted light ( ) it was of a *ruby* colour and that through all depths of fluid.

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14899. To-day the deposit LVIII*b* is settling rather fast from its third water—is clear about $\frac{1}{3}$ of depth from top—then there is a violet cloud there and below that the brown opaque part as seen by reflected light. The deposit LVIII*c* is settling also and pretty clear near the top, below that is the violet cloud to the bottom, there not being much gold in the bottle. By pure transmitted light the fluid is blue. A fine effect is obtained by throwing the cone of sun's rays into the lower turbid part and looking from the front after the cone or along it. Just within the fluid the light reflected was bright and yellow, but at the distant focus it was a fine ruby blue, the reflected light having to come through much fluid—however, I am not sure of what back ground there was, and must try this again (14926).

14900. As LVIII shewed dissolved gold (14898½), I put some into a bottle on Saturday, 30 Aug.—added a little phosphorus dissolved in sulphide of carbon and agitated well; in half an hour it shewed fresh formation of ruby at the surface—agitated well again and left it until to-day. Found it with gold film on the top and dark ruby below: shook it well up to make it uniform. It was now a dark ruby fluid, and in the black tube ruby in all degrees of concentration and depth, shewing that the reduced gold is at first all in the ruby state—tested by proto chloride tin, there was no gold in solution. When a cone of sun's rays was thrown into it and looked at from the front, it was yellow near

the surface, but a beautiful ruby at the focus when in some depth. I made this preparation LVIII*d*.

14901. As this LVIII*d* contained no dissolved gold and no blue gold, I added a little salt to some of it; it became blue at once by observation in all lights—and when a cone of light was sent into it, it gave no ruby light. This blue is quite different from the blue of the deposits, i.e. of LVIII*b* and LVIII*c*. In a few hours the blue gold had deposited and the fluid was clear. Salt seems to take away that reflexion from the particles of gold which enable them to give ruby or amethystine colour, and it also seems to cause their rapid aggregation or condensation, for it then settles rapidly.

14902. When LVIII fluid, containing dissolved gold, had salt added to it on Saturday, the colour changed to blue. Being left until to-day, the colour was gone and there was a small quantity of blue sediment. Gold was in solution, but I cannot say the salt has caused solution, because there was dissolved gold in the original LVIII.

14903. The fluid labled LVIII*b washing* has a faint ruby tint—a deposit brown yellow by reflexion but violet by transmission, and contains mouldiness. Stirred up the whole and run it through a plug of wet cotton in a funnel, which removed all the mouldiness and let the gold particles and fluid pass. It was now a good violet fluid, or rather, by pure transmitted light a deep blue. I do not think the microscope can shew these particles that cause blue but must try.

14904. Can clean mould out well by the cotton plug.

14905. Now proceeded with these lights over the rest of the fluids and deposits, cleansing and arranging some and dismissing others. I will enter them in numerical order.

14906. Fluid XXVII, made 6 Feb. (14342) and described 14859 six months after, is a fine pale ruby fluid over a deposit. I syphoned off the fluid and kept it as XXVII still: it was ruby by transmitted light in all depths of tint—it had a little gold in solution. The deposit was blue by transmitted light and by diffusion in water made a fluid also blue by pure transmitted light in any depth. Made this deposit, after adding water to wash it, XXVII A.

14907. XXX are from a ruby fluid diluted and boiled (14453,

549). XXX A is the deposit and distilled water left to settle in a flask, and XXX B is the fluid from over deposit XXX A. As to XXX A, whilst in the flask undisturbed, the lens like deposit looks ruby or black by transmitted light—the fluid above being ruby. Syphoned off the fluid—it was scarcely affected by proto muriate of tin or nitrate of silver, and scarcely affected litmus paper. It continued ruby by transmitted light in the dark tube up to obscurity—it was continued as XXX A. The deposit, seeming ruby in some parts and blue in others by transmitted light, being mixed with water, gave a fluid beautifully violet under common observation, but in the dark tube was blue with some violet or amethystine light. It is probably a mixture of the so called blue and ruby particles. Made it XXX C.

14908. The fluid XXX B is a glorious ruby with a deposit appearing ruby at the bottom. Syphoned off. The fluid was then found ruby in all depths—is acid and contains no gold dissolved—chlorine in combination is there. No doubt the acids present are hydrochloric and phosphoric acids. It was retained as XXX B. The sediment from beneath it is a *ruby sediment*, and mingled with water gives a ruby fluid in the dark tube by transmitted light—not so pure a ruby as the fluid but still a fine colour—in fact the particles in it, though small enough to produce the ruby effect, are not so small as the particles still suspended and hence the more perfect uniformity or clearness. Put distilled water to it and made it XXX D. It was the first ruby deposit I have obtained and it is as well to remember that the original fluid has been boiled. Probably boiling gives a degree of permanence—and therefore a ruby fluid prepared and boiled at once would most likely be the best means of obtaining abundance of ruby deposit.

14909. XXXV is a ruby fluid, taken some time ago from over a deposit, XXXI, which itself was formed by mingling XXIV, a ruby fluid, with 3 vols. of water and boiling. It has now a little deposit which, being separated, was blue by transmitted light, and with distilled water was made XXXV A. The fluid itself, XXXV, was a fine ruby in the dark tube in all depths.

14910. XLII is a ruby fluid made 12 March (14535, etc., 14867) and is now over a deposit. Separated the two. The fluid was acid—shewed chlorine by Nit. Silver—contained no dissolved

gold—was ruby by transmitted light in all depths—was retained still as XLII. The deposit was blue by pure transmitted light—water was added to it and the preparation made XLII A.

14911. LII. Has a doubtful history, see 14697, 706, 59, 74, 84, 5, 869. Is now a ruby fluid with a deposit looking blue below and has also mucus below. Syphoned off the fluid and left it LII. There was no gold in solution—it was faintly acid—nitrate of silver shews very little chlorine. I think it must have been a deposit washed—it can hardly contain the salt spoken of in 14697. It was ruby with a tinge of violet—in the dark tube it continued ruby until opaque. The deposit was the usual light brown gold, but a pure blue by transmitted light—with water it gave a fluid having a good ruby violet tint by mixed common light; it was made LII *a*.

14912. LV *a*. Was a beautiful ruby preparation with a deposit, in an open glass covered over with a beaker. It seems to be the fluids of LV, LVI and LVII (14769, 871)—is perhaps the deposit from that fluid with some of the fluid left. Fluid separated and made still LV *a*—it has but little colour or gold. The deposit made LV *a* A—with water makes a beautiful ruby fluid—not so clear as LXIV or LXV. The deposit with little water in dark tube was not blue nor ruby but amethystine.

14913. LX *a* and LX *b* are from a ruby preparation made in a basin on the 6th June, 14782, 7. On the 9th Aug. it was separated into a fluid, LX *a*, and a deposit LX *b* (14874). Now resumed. The LX *b* is soon done, being a little fluid with sediment below—the sediment is blue by pure transmitted light, as might be expected, for it is that which was quickly deposited. It probably has not changed in character by time, for when shaken up and observed by general light both reflected and transmitted at once it gives the violet tints, and so differs from the blue deposit produced by salt. It is still LX *b*.

14914. LX *a*, the fluid, had much floating mucus and a deposit, the fluid itself being ruby. Took out the mucus by a deal splinter without shaking gold out of it—it was blue violet in the water and contained very few particles of gold. Being put with water into the dark tube—it was blue by transmitted light. Syphoned off the fluid—it contained no gold in solution—was slightly acid—

contains chlorine in combination. In the dark tube it was ruby in every shade of tint up to bright sun light. Was still kept as LX*a*.

14915. The deposit was made LX*c*, and must be a finer deposit than LX*b*, or rather a slower one. By pure transmitted light it gave a pure blue colour. When mingled with water, and reflected light allowed to come in, it gave quite a ruby violet for instance when looked at from the front but with a white foolscap back ground. When the back ground was changed for black, the reflected light was a pale brown. Can hardly think that it and the transmitted blue would produce the ruby violet by mere mixture of the rays unless there was some influence of one on the other besides mixture. Go further into this (14926, 7, 8, etc.).

14916. LXI is a fine ruby fluid made by shaking sol. of gold with phosphorus and Sulrt. carbon in a bottle (14829, 43, 75) made June 30. It is an exceedingly fine ruby fluid–there is no deposit except of excess of sulphuret carbon, phosphorus, etc. It was decanted off from that. In dark tube it was a fine ruby to all depths. Probably boiling would cause it to give a ruby deposit. It is a good specimen.

14917. LXII. Gold solution and Sulrt. Carbon; no phosphorus (14830, 43, 76). This fluid has a deep blue deposit and a supernatent amethystine fluid–the whole shaken up gave a fluid brown by reflected light, deep blue by transmitted light–the deposit of gold seems good but there is little appearance of ruby–blue predominate[s]. This was ruby at first–what tint will sun focus give in it? (14933).

14918. LXIII. Excess of acid in the gold solution (14831, 43, 77)–is as it was before, 14877, fluid nearly colourless, deposit phosphorus and gold, blue.

14919. LXIV. Amethystine fluid, 14878–passed through cotton to clear off mucus–the fluid is ruby in the dark tube by transmitted light–yet amethystine in the bottle (14934).

14920. LXV. Made? Is a ruby fluid and deposit, see 14879. Beautiful supernatent fluid–seems very clear–but have no sun at present. A deposit below looking ruby whilst in the bottle. Separated the two. The fluid made LXV–considerably acid–contained no dissolved gold–nitrate of silver gave presence of chlorine. Was ruby in dark tube as long as light could pass. The

deposit filtered through cotton and diffused in water was ruby violet in dark tube by sun light, until opaque—as if particles were either intermediate in size between the ruby and the blue, or were a mixture of the two; made it LXV*a* (14935, 6).

14921. LXVII. Ruby from 4 leaves of gold (14890). Was decanted off from deposited excess of phosphorus—was a very fine dark fluid—which in dark tube was amethystine in small quantities and ruby in larger up to obscurity. Appears to be no blue particles here as yet. Will time cause them to form? Or boiling?

14922. Dismissed the following:

XX.	XXV D.
XXI.	XXV L.
XXIV—in tube.	XXV—in tube.
XXIII—in flask—blue—mucus.	XXXII.

14923. The blue gold or fluid transmits blue light, whatever the thinness or thickness of tint. The ruby fluid transmits ruby light up to greatest thickness, becoming perhaps amethystine when very perfect. Could be no reflected light in either of these cases, except such as was thrown on from particle to particle; perhaps that not impossible, but if it occurred at all, it would seem to indicate a peculiar reflexion in the particles sending the light on.

14924. As to the rosy hue of a fluid blue by transmitted light, the light reflected from the gold within, when looked at obliquely, is very different in effect to white light coming through from the other side yet in the same direction. What would gold light coming through do? Must examine the lights by a prism, especially the light of the lens focus.

14925. XLIV, see 14868, is a dark deposit and a clear colourless fluid. By nitrate of silver there was so much chlorine as to shew salt in the fluid. I have no doubt it has been mixed with XLV and made one result.

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14926. Deposit LVIII*c* examined again (14899). Had settled since the 2nd. When shaken up in the bottle it gave a fluid, reflecting pale brown light but transmitting a pure blue light from the clouds, using the bottle as vessel. When the sun's image

(reflected) was looked [at] through the fluid it gave blue light, with development of red parts here and there from prismatic action and the imperfection of the eye. When the sun's light, reflected from a *leaf of gold* laid upon glass, was sent through it to the eye, the light was also blue but with more red in and about it.

14927. The cone of Sun's rays by the lens was sent into the fluid and looked at from the front, i.e. the lens side:—the cone light was yellow near the surface of the fluid but amethystine from the deeper parts up to the focus. This effect was beautifully seen also as the focus was made to fall nearer to the surface or farther in. When the focus is thrown across and *through* on to white paper there, the light on the paper is blue but that which comes back to the eye through the fluid is amethystine. A more dilute proportion of the fluid gives like results.

14928. Some of this fluid was put into the dark tube ( ) and different coloured objects examined through it. Over white paper the tint transmitted was pure blue—over the hand, the transmitted light was reddish blue—over the gold leaf on glass in common day light, the transmitted light was amethystine or violet warm—over red Morocco, the light was very red or ruby. In fact the fluid transmits red rays very well. When some of this fluid, LVIII*c*, was placed in sun light, so as to reflect its brown colour, and was then looked at through the fluid in the tube—the colour was a good amethystine tint just like some of the former specimens. So that whether the reflecting gold is in or out of the fluid makes no difference. The rubification of the fluid is due to the mixture of red rays with the blue.

14929. Threw the Solar spectrum on to white paper, in a light room however, so that spectrum was mingled with the white light of the paper. Looked at this spectrum through the blue fluid, i.e. LVIII*c*, in the tube; the blue rays passed beautifully—the red considerably—the green somewhat—the rest were obscured by the white light from the paper with its general illumination. Try it in a dark room.

14930. So the cause of the ruby or amethystine produced by these fluids, looked at generally, when they are really not ruby by transmitted light, is apparent, being due to the mixed trans-

mitted blue rays and reflected red rays of the gold. As a whole, such a fluid as this transmits blue rays best, then red rays–aftwds. other rays. Of course, other proportion[s] of blue than these in white light would transmit other final colour than blue. So a blue fluid in the dark tube, looking at gold surface reflecting light, can give a ruby or violet ruby to the eye.

14931. The fine ruby fluid LXI (14916) is deep in colour and yet apparently very clear by comparison with others, i.e. very little turbid. The sun and lens shews a feeble cone in it. If there is no gold in solution, the division must be exceedingly fine, perhaps the extreme case, and the quantity of gold necessary in that extreme state of division to give a red or ruby tint will be extremely small. Can tell this by proto chlo. tin. It smells a little of phosphorus. In the long dark tube the fluid was always ruby by transmitted light. When the spectrum (14929) was looked at through this fluid, the red passed well–but very little green passed and no sensible blue.

14932. Some of this ruby LXI had salt (a little was not enough) added; it became a fine blue; then the Sun and lens gave a feeble cone as before, but with no sensible ruby tint. Being put into the dark tube–then over white paper, it was pure blue–over the hand and Morocco, amethystine and reddish tints passed as before (   ), but slightly. Looking at the Spectrum, the blue and green was transmitted well, the red scarcely. The sun's image reflected from the gold leaf on glass seemed all blue. This salt blue seems to transmit the red rays but not so well as the former unsalted deposit. Neither does it reflect the red rays in any degree like that unchanged deposit.

14933. The fluid LXII (14917) put into the darkened tube, when used over red morocco–gold leaf–and reflected Sun light–was amethystine to the end, transmitting some red rays as the other blue fluids did.

14934. The amethystine fluid LXIV (14919). It is very pale and clear but the sun's rays and lens shew the visible cone. In the short obscure tube–it was amethystine over white paper for full length of the tube–placing gold leaf–morrocco–the hands, etc.–the different coloured rays passed well and made the tint more amethystine or ruby. When in the long tube and examined by

the sun's image reflected by silvered mirror, the light was a very fine ruby with no approach to blue. So LXIV is an amethystine fluid which in increased depth passes into ruby. Do all true ruby fluids become violet by dilution or thinning?

14935. Ruby fluid LXV (14920) is also a very fine clear fluid; in that respect like LXI (14931). So examined it by the sun's rays and the lens. Looking from the front, the near particles were apparently greenish yellow, those more distant in the fluid ruby or amethystine as in the other cases. Being put into the long tube and the sun's image reflected looked at through it, the colour was a beautiful ruby–no approach to blue, violet or amethyst.

14936. The deposit LXV*a* (14920) partly deposited since the 2nd. The sun's rays and lens gave a cone yellow near the surface and amethystine farther in. Poured off the upper part, and it in the dark tube gave over white paper an amethystine blue and over gold leaf, morocco, etc. more ruby tints. The deposit part over white paper was a pure blue.

14937. My piece of ruby standard glass (   ) gives a ruby transmission of light like LXI. No signs of blue transmission appearing. It is very possible that some specimens may transmit some blue light.

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14938. The Ruby LXI was tested by proto chlo. tin for gold in solution (14931) but there was none there. The ruby LXV was tested in like manner, but it was free from dissolved gold.

14939. Some of the ruby LXV had on the 4th a little salt added to it–it became blue–it was boiled in a tube and the signs of gold colour disappeared–it was left until to-day. By the sun's light and a lens, particles of gold were found in it having a brown tint. Examined by proto chlo. tin, but there were no signs of solution of the gold, 14789, 90, 1, 2. Some of the same LXV ruby had pure Mur. acid added; it became blue and pale–it was boiled also on the 4th and to-day examined–there was no gold dissolved and by the sun's rays and lens the particles of gold in suspension gave a brown reflexion (see 14817).

14940. Some of the ruby LXV had pure Mur. Ammonia added to it on the 4th; it soon turned purple. Examined to-day, there was no colour, either ruby or violet in the fluid; all the gold was below as a purple precipitate.

14941. The ruby fluid LXI, smelling of phosphorus—a little of it was put into a large bottle and agitated with air, to oxidize the phosphorus—the smell was soon gone but the fluid did not change in character. Indeed the stock bottle of it is now nearly free from phosphorus smell—it has been so often opened.

14942. The Ruby LXI was well boiled on the 4th in a flask and set aside (14916). To-day examined. It is darker and more amethystine than before—has a little flocculent amethystine deposit. The lens cone is as visible in it as in the unboiled portion—about the same degree. In the long dark tube this fluid was a fine amethystine ruby. When poured out of the flask, it left an amethystine film adhering to the glass, which resisted Mur. acid but dissolved in N.M.Acid, giving the ordinary solution of gold. The clear boiled fluid was made LXXVI.

14943. The ruby LXVII was boiled also on the 4th in a flask for some time. To-day examined. It is changed to blue almost or blue amethyst, i.e. in the same direction as the last. There is no sensible deposit either loose or onto the glass—the sun's rays and lens give a cone about as the unboiled. In the dark tube it is amethystine blue over white paper. It was made LXXVII. When the unboiled LXVII was examined in the dark tube, it was fine amethyst over white paper. The lens gave a feeble cone only and there can be but very little gold in this preparation.

14944. A little pure Mercury was on the 4th put into a portion of the ruby LXV and well shaken frequently; it appeared to have no effect upon it. To-day examined: is still the same though often agitated—no action of the mercury on the gold—no contact. The preparation was made LXXVIII.

14945. XLII ruby had a little caustic ammonia added to it—it became a little bluer at once, but not rapidly. Was made LXXIX and left.

14946. On the 4th, 12 drops of De la Rue's solution of gold (   ) was put into 60 fluid ounces of distilled water for a common solution. Being examined by the Sun and lens, it gave

no sensible cone, i.e. there was no gold except what was in solution, and no other kinds of diffused particles.

14947. A portion equal to 6 fluid oz. was put into a clean bottle—a few drops of phosphuretted ether added and the whole shaken—it became a fluid red at once, of a garnet colour. It was made LXVIII and left. Being examined to-day, it was of a deep amethyst throughout—there was a little deposit and the lens gave a feeble cone; being shaken up there was more light in the cone. This fluid in the dark tube was of a very fine ruby over white paper or light. The ruby formation is very fine and ready in this way, and probably the extreme state of division of gold is produced at once.

14948. A like portion of the solution 14946, put into a bottle with some scrapings of clean phosphorus, became amethystine near the bottom only upon being left until to-day. Now shaken up many times—the action is very slow and the cone by lens and sun's rays is very faint as yet. Made LXIX.

14949. A like portion of the solution 14946 put into a bottle and a little sul. hydrogen gas thrown in to the atmosphere and shaken—immediate change and brown colour produced, not deepening any more by time or by the addition of more sulphuretted hydrogen. It is in the same state to-day—a pale gray brown; lens and sun light gave a faint cone and floating sparkling particles strongly reflecting light. No signs of either pure ruby or blue gold. Was dismissed. Made for the time LXX.

14950. A like portion of the solution 14946 had two drops of brine of *pure salt* added to it and then the solution of phosphorus in Sulrt. carbon added from end of a glass rod and all well shaken up and left at LXXI. To-day the fluid was a pale ruby above and a beautiful ruby at the bottom for about one third of an inch—being shaken up, a fine ruby tint was produced—by the sun and lens a good cone was produced, which looked at from the front, was yellow in the near parts and ruby within. This ruby tint is different to that made with the solution of phosphorus in ether (14947). To-day a second like preparation was made but in which the salt was thrice as much: it was marked LXXI A. Another like preparation was made in which the salt was six times that in LXXI: this was marked LXXI B. All were left.

14951. A like portion of the solution 14946 was put into a bottle on the 4th with the addition of a little Tartaric acid and made LXXII. To-day the fluid has a pale violet or amethystine colour—there is a darker deposit below which shakes up into turbidness. The sun and lens gave a cone yellow near the first surface but amethystine further in. In the long dark tube and with reflected sun light, it was of a fine ruby colour.

14952. A like portion of 14946 in a bottle on the 4th had a dozen drops of the shelf preparation called Aldehyde. To-day there was no change and doubt the Aldehyde preparation. Made it however LXXIII.

14953. A like portion of 14946 in a bottle—a rod dipped into solution of proto sulphate of iron—dipped into the solution with instant agitation—there was a bluish effect produced at once: made LXXIV. To-day, the fluid is green and there is a deposit of gold, brown by reflected light; the gold was in small flocculi, and being shaken up, made a fluid altogether greenish. By the Sun's rays and lens a cone was produced, but the reflected light was a clear red brown, not the yellow. By transmission, the fluid was dull green, and in the dark tube a beautiful green—including at the time the particles.

14954. A like preparation was made, but with more sulphate of iron—it was twice added by the rod; the change was instantaneous and the fluid became a fine peculiar green: it was made LXXV. To-day the fluid is clear and has scarcely a trace of green, but there is a deposit in flocculi which by reflected light is of a brown colour and by transmitted light of a fine green colour, like pressed or hammered gold. This deposit is flocculent, shewing an adhesion between the particles. When agitated and examined by sun's rays and lens, they reflect a peculiar bronze red. When the cone is sent into the fluid the near part is brown, but the deeper parts green, as the reflected light becomes affected by the transmission through the intervening fluid. The stirred up fluid in the dark tube had a fine green colour. The green colour of the gold in this state is very remarkable, so like that of pressed or beaten gold.

14955. The Ether phosphorus preparation LXVIII (14946, 7) is of a dark purple blue colour and continues so in the dark tube—there is a little dark flocculi deposit which was separated and thrown away. It smells of ether—a little in a capsule over the sand bath deposited a dark blue grey film as the liquid diminished. There is no dissolved gold in this fluid, and I think it is a *very finely divided purple gold.*

14956. The phosphorus scrapings LXIX (14948)—goes on very slowly; the fluid is faintly amethystine ruby at the bottom where the phosphorus lies. Shook it up and left it.

14957. *Salt in the gold solution* before hand, LXXI (14950). This preparation is a fine ruby every where, but half an inch at the bottom is of a deeper tint than the part over it; it also is apparently clear, whilst the part over it is sensibly turbid and by reflexion is full of brown particles. The whole is ruby by ordinary transmitted light. When examined in the sun light by the lens, the same difference appears, and when the upper part was separated, the clear bottom part was really deeper in colour by transmitted light than the part above it. I think that probably the cause is the presence of phosphorus particles below and the slow mixture of the different parts. The segregation of the gold into particles in the upper part was distinct; the change had happened there which had not happened below. Put all together, agitated and left it LXXI as before.

14958. The preparation with more salt, LXXI A, 14950, is altogether darker than the former, LXXI—it has a slight film of gold on the top. It is a dark purple, and the bottom half inch as in the former case far darker than the rest (3 or 4 inches) above it. The bottom is opaque to white paper light, but of the same amethystine colour as the rest by abundant light. Both it and the upper seem free from turbidness, whatever the lens may shew. In general sun light, there are the same appearances—the lens cone looked at from front and side was more luminous in the upper than the lower part and could be followed further in—but the form of glass at the bottom was inconvenient. The whole wants agitation and mixture. Agitated and left it.

14959. The preparation with still more salt, LXXI B (14950). Was altogether blue—the fluid was weak in colour but there was

a deep coloured deposit as a coating on the bottom glass; this coating was blue by transmitted light and so was the fluid. In sun light the lens cone appeared faintly, some gold being still in suspension. Decanted the fluid—there was no gold dissolved in it and very little diffused, so *dismissed it.* The deposit adheres to the bottle in flocculi—is dark blue by transmitted light—black by reflected light. It was shaken up with distilled water and left. As regard the quantity of gold left here, there was only 3 oz. of gold fluid originally instead of 6 oz. in the other two preparations with salt.

14960. The Tartaric acid preparation, LXXII (14951). There was the fluid—a film on the sides of the bottle and a dense deposit. The fluid had no colour, contained no gold either diffused or dissolved, and was dismissed. The deposit was shaken up—was in dark particles, visible, and being turned into the dark tube with water, gave poor colour, only an obscure blue—it was also dismissed. The film on the sides of the bottle was a *good amethystine ruby*, and being washed, the bottle was filled with distilled water and kept as a ruby film preparation.

14961. The supposed Aldehyde preparation, LXXIII (14952)—was still in the original state—no change to colour—gold still in solution. So dismissed it as LXXIII, and for a trial put Ether phosphorus to it—it immediately changed, as LXVIII had done. Made it LXXIII.

14962. The sulphate of iron preparation, LXXIV (14953). A clear fluid and a light brown deposit, green by transmission. The fluid drawn off has gold still dissolved in it by proto chlo. tin—by Ether phosphorus—and by proto sul. Iron; added sul. iron to it—became blue in tint—the general reflexion very brown both in common and sun light. Made it LXXIV A.

14963. The deposit of LXXIV was by transmitted light either dark or green and of a good colour. Shaken up in water it was brown by reflected light. Some in the dark tube—common salt added—caused no change. Hydrochloric acid changed it slowly and by heat quickly—a yellow solution was formed and condensed black gold left. No gold was dissolved, but iron only, which had been associated with the gold. Nitric and sulphuric acids produce the same effects. Potash did not affect the green

deposit, except that when boiled it perhaps made it denser. M.A. coming after acted as before. So the green precipitate is an association of gold and iron—probably metal gold and peroxide of iron. All the deposit used up—so LXXIV dismissed.

14964. The preparation LXXV (14954) with much proto sul. iron -the fluid drawn off had no colour—a trace of gold in solution—also a trace of iron in solution—by lens, little or no diffused gold—it was dismissed. The deposit was more than for LXXIV, and of a warmer green—washed in much water—whilst diffused was red brown by reflected light and green by transmitted light. Then 4 or 5 drops of dilute S.A. was added and the whole fluid, being acid to litmus paper, was left as LXXV. It gradually changed, clearing, and will probably become as LXXX (   ). This is just as before, an association of Gold and Iron.

14965. The boiled ruby LXXVI (14942) is still amethystine—no signs of deposit as yet. Must leave it for length of time.

14966. The boiled amethystine LXXVII (14943) dark amethystine blue—no deposit as yet—must leave it.

14967. The ruby with mercury LXXVIII (14944) is unchanged.

14968. The Ruby with ammonia LXXIX (14945), ruby violet—no settling.

14968½. Precipitated by proto sul. iron from an *acid* solution of gold. 6 fluid oz. of the solution (14946) had 5 drops of M.A. added—it was acid to litmus—then proto sulphate of iron added by the glass rod as before (14953)—no immediate precipitate as before—added more sol. iron—an effect came on—a feeble brownish reflection—the sun's rays and the lens gave signs of diffused particles, but they were in a very condensed state. There is very little colour in the fluid, but yet a certain feeble *amethystine* appearance, *not a green*—yet all the gold is down, for there is none in solution. To a little of it more proto sul. iron was added, but it did nothing. Made this preparation LXXX and set it aside.

14969. So this is the true history of the iron action. The precipitated gold is green only when it takes down iron with it; and is, if any thing, amethystine if it goes down without iron.

14970. A portion of the gold solution (14946) was made feebly acid, and divided into two parts—then one was precipitated by proto sulphate of iron and the other by phosphorus ether. There

was scarcely any sensible colour change in the former, whereas the latter gave a deep tint. Makes a good illustration of the difference due to the gold as divided by the two methods, and of the effect of finely divided gold upon light.

14971. Twelve fluid oz. of the gold solution (14946) had 12 drops of De la Rue's solution of gold added, so as to make a strong solution by comparison with the former. Phosphorus in Ether was added in successive portion[s] with agitation. The object was to see if gold would aggregate and make *turbid* fluid, and also, if so, to prepare an Ether deposit. The fluid quickly became a dark purple or amethyst colour—it still gave a further darkening effect by proto chloride of tin, as if gold yet in solution. It was made LXXXI.

14972. Some of the gold solution (14946) had a little ether added and, after agitation to dissolve the ether, a piece of phosphorus was put in. But the action was not sudden or good—agitated and left as LXXXII.

14973. Wanted more of the Sul. carbon Phosphorus preparation, so took 18 fluid oz. of 14946 solution; added 18 drops of De la Rue gold solution and then the phos. in Sulrt. carbon in successive portions, agitating. Soon became a deep ruby. It was left, *gold being yet in solution*, as preparation LXXXIII.

14974. Endeavoured to transfer some of the sediments into other media than water. Took deposit XXXC (14907), which had a little almost colourless fluid over it, but was itself well settled—decanted off the fluid and still kept it as XXXC for a type of the colour character. The deposit, which before disturbance was ruby at the edges, when mingled up with the little fluid still about it, was amethystine at the edges and blue in the middle. When mixed up together, this was the case, shewing the effect of attenuation at the edges or deepening in the middle with a fluid *uniform* in colour and character, as far as agitation could make it so. Added a drop of alcohol and agitated—mixed well in—no sensible change—added more alcohol—then more—then more, agitating always until the alcohol was probably 50 fold the bulk of the water originally left with the deposit. The colour continued blue by transmission, but I think it lost character—as if the gold particles were aggregating. The whole preparation seems

to approach LXXV and LXXX in appearance, but we shall see what comes of it. Made it XXX E.

14975. Again, LII A (14911) is a deposit with weak fluid over it—decanted the latter and left it as LII A *m*. The deposit with a little water is blue by transmitted light. It was divided into two equal parts. One had a certain volume of water added to it and became a blue fluid of certain intensity, marked for the time LII A 1. The other part had an equal amount of absolute alcohol added to it and was made LII A 2. They appeared of about equal depths of blue colour and were left for the action of time.

14976. Again, LX*c* (14915) is a deposit with pale fluid over it. Made the fluid LX*c m*. Added alcohol to the deposit—the reflected brown of the gold became darker—the transferred colour was a fine blue. Set it aside as LX*c* still.

14977. Endeavoured to divide gold leaf into fine particles, by grinding it in an agate mortar with a little water and calcareous spar and then dissolving out the carb. lime by a little M.A.—the particles left with water in the dark tube gave a very poor result—an obscurity but no certain colour. Ground some gold leaf up with honey—had a better result—a pale green tint was transmitted, but obscure, dirty and poor.

14978. The deposit XXXV A (14909) had the fluid drawn off close and then it was rubbed up in the agate mortar with calc spar in hopes of impressing the effects of pressure on to its particles, being well rubbed and the carb. lime dissolved by M.A.—the powder with water was examined in the dark tube. The colour was *blue* as before, but very much less. Probably the contact of the chloride of calcium alone would cause condensation, etc. of the particles of gold. The unused portion of the deposit and the fluid at first over it were put together and left still as XXXV A.

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14979. The *preparation with salt*, LXXI (14957), was of a fine ruby colour—there was settling and clearing at the top—and by turning up the bottle there was evidently a little sediment at the bottom of a purple colour; the whole was agitated and left. That

with more salt, LXXI A (14958), was of a deep amethystine colour, settling also at the top and with very little sediment at the bottom—it was shaken and left. That with most salt, LXXI B (14959), had the fluid nearly clear of colour and a marked blue deposit—it also was left. The three shew very well the progressive effect of the salt.

14980. The sample LXVIII was a ruby from phos. in ether (14955). To-day it is a dark blue all through—and as yet no deposition—it is a case of fine blue division. LXXIII (14961) is also now a phos. ether preparation—it is blue at the bottom for half an inch and ruby above—the blue being a sediment, which floated up as the bottle was turned up.

14981. The prep. with proto sul. iron and dil. S. Acid, LXXV (14964), is a clear fluid of no colour, and a thin brown adhering deposit at the bottom. The fluid was poured off and dismissed—the deposit washed first quietly and then by agitation; the part which was loosened made a feeble diffusion in the fluid, transmitting a dirty blue colour in the obscure tube—but the gold is dense and in largish particles and flocculi. Phenomena of colour are nearly gone and no signs of the green translucency remains.

14982. The acid gold sol. with proto sul. iron, LXXX (14968[1]), has a clear fluid with no colour and no dissolved or diffused gold—it was poured off and dismissed. The gold was left adhering to the sides and bottom of the glass—that on the sides produced much ruby effect, that at the bottom was by reflexion a pale brown. When shaken with water it did not easily come off; such as was diffused gave to the water in the dark tube a very faint blue tint. Water was left with it and all kept as a specimen LXXX.

14983. The strong gold preparation by Phos. Ether LXXXI (14971) was very turbid, and red brown by general reflexion. The top was clearing but clouds rose up into it on moving the bottle. By transmitted light the fluid was amethystine ruby in all parts. There was no gold in solution. At the bottom there were some coarse particles which, being removed, were found to be clots of gold with a little phosphorus; when they were shaken with water they produced a diffused fluid, blue in the dark tube.

[1] Should be 14968½.

All being shaken up and then the uniform fluid taken from over these particles, the former was continued as LXXXI. Took a part of LXXXI and added more phos. in ether to it–it seemed to deepen the red brown of the gold deposit at once–made it LXXXI A.

14984. The strong gold prep. by phos. in Sul. carbon, LXXXIII (14973), was very red brown by reflexion–had a film, slight, on the top–was then clear ruby for half an inch down–then very turbid–and at the bottom, half an inch not turbid but clear again–quite at the bottom some blue deposit. All was shaken up together and then it was ruby amethystine, smelling of phosphorus–some particles soon settle which look like phosphorus and gold. It was set aside to stand. A portion of it was put into a bottle and a little Phosphorus in Sul. Carbon added to see if further effect: it was made LXXXIII A.

14985. Gold sol. and piece of phosphorus, LXXXII (14972), goes on slowly–it is amethystine.

14986. My solution of gold, XXXIX (14291, 483), was dismissed. It was very acid and contained much sub-chloride of gold.

14987. The deposit with Alcohol XXX E (14974). Looked very thin, as if the gold compressed–red brown by reflected light–not much settlement. In the dark tube the liquid was a feeble but pure blue–gold has become condensed here. This gold look[s] very like [that] of LXXX from acid and iron, except that it does not settle so soon. Dismissed the fluid, XXX C (14974).

14988. Another deposit put into Alcohol, LII A 2 (14975), with a corresponding portion reserved in water, LII A 1. Both were amethystine and much alike–they were passed through cotton to remove portions of mouldiness–both gave pale blue fluids in the dark tube. The 2 deposit sticks rather to the glass. So Alcohol wets the particles and seem not to differ much from water in its action on them. Dismissed the fluid LII A *m*.

14989. Another deposit in Alcohol, LX*c* (14976). The alcohol clear–the gold all at the bottom–brown by reflexion, by transmission a dark imperfect blue, for metal now clotted and aggregated. LX*c m* was amethystine–but I threw it away.

14990. After a time, when LII A 2 (14988) had settled, which it soon did–poured off the Alcohol closely and then put on

camphine in small portions, agitating. The gold did not change, but it did not diffuse in the camphine or wet with it. It seemed to separate a small portion of water from the alcohol and form a damp film which stuck to the bottom and sides of the bottom. This gold was brown by reflected light and a dull dirty blue by transmitted light. Left it as LII*a* 3.

14991. Then took LX*c* ( ) and dealt with it in the same way. The camphine and gold would not touch—the camphine took the Alcohol, the gold clung to water, became condensed and stuck to the glass as a wet film, poor in effect but still transmitting blue light.

14992. LXXIV*a* (14962) is a sulphate of iron preparation—to-day the fluid above was clear and a deposit brown by reflected and green by transmitted light was below—poured off the fluid—added M.A. to the deposit; it dissolved iron and left dark heavy gold as before—dismissed the whole.

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14993. The preparations with salt—LXXI (14979): it is still a fine ruby—deep colour—settling, and has some deposit at the bottom. In sun light is rich in suspended particles and gives an excellent cone of rays. LXXI A, with more salt, is of a deep violet blue—and presents a good cone of light—there is a blue deposit at the bottom. LXXI B: is altogether blue, but there is little colour in the fluid—the gold had gone down as a blue deposit; this is the most salt.

14994. Reduction by Tartaric acid (14960). No gold in solution. By sun light and lens none in suspension. All deposited, but it is in a dense state, yet not blue but as a ruby or amethystine film adhering to the bottom and sides of the bottle, LXXII.

14995. The two boilings LXXVI and LXXVII (14965, 6). The original fluids, LXI and LXVII, are as they were, deep in colour and no deposit in them. But LXXVI has its gained amethystine colour—is settling—a little at the top clear—the rest deeper in colour. Sun cone very fair—a little amethystine deposit at the bottom. LXXVII: is Amethystine blue—settling—about $1\frac{1}{4}$ inch nearly clear—then about $2\frac{1}{2}$ inches more cloudy, uniform but still thin in gold. At the bottom a deep blue violet deposit—adhering

to the glass—it is a good blue violet by transmitted light. The sun cone less than in LXXVI.

14996. The Ammonia Ruby, LXXIX (14968). Fine amethystine violet in colour as before—settling—nearly clear at the top. At the bottom a mouldy flocculent deposit—passed it through cotton—mould left amethystine blue. The loose deposit and the fluid amethystine, even when in depth—becoming neither blue nor ruby.

14997. Strong ether phos. preparation, LXXXI (14971, 83). A fine ruby fluid with clouds. Deposit at the bottom blue by transmitted light, but in thinner parts ruby. Shook up, then left to settle. LXXXI A (14983), to which more phos. ether had been added, had now a clear fluid. The deposit at the bottom was blue and much aggregated—by shaking up it gave a fluid of a dull blue obscure colour by transmitted light—by standing, the deposit soon settled again.

14998. The strong prep. with Phos. in Sul. carbon, LXXXIII (14984). This a fine ruby fluid—dense—turbid—a strong red brown by reflected light—a deposit at the bottom—it blue by transmitted light. Shook it up; there was excess of phosphorus. After some hours decanted off the fluid and kept that as LXXXIII. The sediment with water passed through cotton, to clear off the phosphorus and coalesced portions of gold, and make LXXXIII B, as the first sediment from this preparation.

14999. LXXXIII A with much phosphorus (14984) is of a beautiful ruby amethystine colour—has a blue deposit. No harm done by the addition here—but in the LXXXI A or ether case, the separation of the gold has been hastened.

15000. As to the question whether concentration of a colour proceeds always in one direction, i.e. from blue or violet, etc. towards ruby (14934), examined some fluids. XXX B is a good ruby and becomes a glorious ruby by increased quantity and increased light; by dilution or rather diminution it became violet. LXXI is a fine ruby in depth—a violet on diminution of its mass. LXXXI, amethystine when dense—towards blue by attenuation. LXXXIII a deep ruby—attenuation makes it amethystine and then blue. So the red rays appear to pass most freely—then the blue—the rest being earlier stopped. Try this effect with a deposit of ruby character (15051).

15001. Deposits in Alcohol, etc., XXX E (14987)—very little gold there—is chiefly at the bottom—adhering and dense. LII A 1—the gold is at the bottom, a deep blue—shaken up, it makes a feeble amethystine fluid with the alcohol over it. LII A 2 is denser and has a deposit more like XXX E.

15002. LII A 3. (14997[1]), a deposit and camphine—no gold in the fluid—all adhering to the bottom as a brown film or crust, blue by transmitted [light]—making very little appearance. No cone of rays is produced in the fluid. LX *c* (14991) in camphine was just as before; no association with the fluid.

15003. Change of ruby by Salts. Took the ruby LXVII and tried it with various salts in solution, adding more or less rather carelessly. It was changed to or towards blue more powerfully by some than others. The Nitrates of Potash, Soda, Magnesia, Manganese and Baryta. The chlorides of lime, strontia, Manganese—the sulphates of Magnesia, manganese and lime—the acetates of potash, soda, zinc, lime—effected the change freely. The sulphate of soda—Phosphates of Soda and potassa—Chlorate of potassa—Acetate of ammonia—with Boracic acid and Sulpho-cyanic acid, more feebly. The hypo-sulphite of soda, moderately.

15004. Mercury and deposit. Took the deposit, LXXXI A (14999), poured off the fluid—added water and a little pure mercury, agitated it to amalgamate the gold—no apparent association. Made it LXXXIV. Dismissed LXXXI A as a preparation.

15005. Some of the solution of gold (14946) with a little pure glycerine put into a bottle—made LXXXV and left. Next day—reflexion brown—transmission blue—no deposit—not much depth of colour—the blue is pale, like an acid iron solution ($14968\frac{1}{2}$)—there is gold yet in solution. Still, there is separation of the gold.

15006. Some of the solution of gold (14946) with a little glycerine were heated and boiled in a tube—it became blue—all the gold separated, but in a compressed condensed state, so as not to give a fluid of any character. Repeated, with the same results. Next day, both these tube[s] had blue deposits as films adhering to the glass, and the fluids were clear.

15007. Some of the solution (14946) with a little sugar, heated in a tube, gave a very fair reduction. The fluid became ruby

[1] ? 14990.

amethystine, turbid, etc., just as by phosphorus in ether. Gold is all out. Next day–settling at the top, the whole is a fine amethystine colour. Two days after last–just the same appearance and colour–settling–23rd, blue deposit–shaken up gave amethystine fluid in the air but in the dark tube was blue.

15008. Introduced heat into the process of reduction. 15 fluid oz. of the solution (14946) were boiled in a flask, and whilst boiling, two or three small portions of phosphorus ether were introduced with stirring–became of a clear ruby–gradually became turbid–gold all seems out of solution–the fluid is by reflexion brown–by transmission violet or amethystine; set it aside as LXXXVI. May be useful. The excess of ether is probably driven off.

15009. 15 fluid oz. of the gold solution (14946) treated in the same way with sol. Phos. in Sul. carbon–changed to a dark amethyst–continued to boil–it gradually cleared to a warmer ruby–not sensibly turbid–floating portions of phosphorus smoking in the steam–then became more dull and of a dirty amethystine colour–gradually flocculi of gold and phosphorus formed and floated about and as a coloured flu[id] the specimen lost its character. Made it LXXXVIII. There is no advantage in using a boiling temperature.

15010. A weak solution of chloride of platinum put into a bottle with a little phos. ether added–agitated and made LXXXVII. A weak solution of chloride of palladium with phosphorus in ether added–change at once–darkening–gradually increased; made LXXXIX–left for examination. A weak solution of proto nitrate of mercury–neutral–with phosphorus ether in a bottle–it changed, becoming brown and was left; made XC.

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15011. LXXXI (14997): strong preparation, Ether Phos. Is clearing at the top–then turbid to the bottom. Drew off turbid liquid; made that LXXXI still. Passed the deposit through cotton with water–made LXXXI A.

15012. LXXXIII. Strong Phos. in S.C. (14998). The dense supernatent thick fluid was separated and made still LXXXIII–the deposit was passed through cotton and added to that made LXXXIII B, whilst the fluid of LXXXIII B was added to LXXXIII.

LXXXIII A was also divided into fluid, which was added to LXXXIII, and a deposit which, being passed through cotton, was added to LXXXIII B. LXXXIII A is dismissed therefore.

19*th*. LXXXIII fluid decanted, and left LXXXIII—the deposit with water was blue in tube and made LXXXIII C. The first deposit LXXXIII B was clearing above beautifully—it was amethystine in the glass but blue in obscure tube in all dilutions—the deposit itself was, in more water, blue in tube in all proportions. The fluid LXXXIII was in tube ruby when strong, amethystine when enfeebled.

15013. LXXXVIII. Gold and S.Carb.phos. hot (15009), a weak purple solution which, accumulated in depth, became dull ruby—the deposit was clotted and black—mingled with phosphorus—dismissed.

15014. LXXXVI. Gold and Ether, etc., hot (15008). Very ruby to-day—very turbid—in clouds—drew off the fluid; put it aside as LXXXVI for a ruby or amethyst deposit. The residue passed through cotton and water added made a violet deposit, LXXXVI A. 23rd Sept. LXXXVI is a ruby fluid—cloudy—settling—decanted the fluid and kept it as LXXXVI. The deposit LXXXVI A was very mouldy—passed it through cotton again and then added the deposit from LXXXVI to it.

15015. *Platinum*, etc. LXXXVII (15010). Pale yellow brown solution and a deposit, partly as brown film adhering to the glass and partly loose. The fluid decanted and left as LXXXVII—the deposit with water made a turbid fluid, brown: LXXXVII A; on 19th, deposit just as before, no cone in the liquid—the fluid LXXXVII clear—no sensible cone—Phos. Ether did nothing more. Same on the 23rd. Dismissed LXXXVII.

15016. Palladium, etc., LXXXIX (15010). Is dark—has settled—much appearance about $\frac{1}{2}$ inch from bottom—very light deposit in weight—looks like an extreme division—drew off the upper part and left it as LXXXIX. Added much water to the lower part and made it LXXXIX A. On the 19th, LXXXIX clear fluid—dark deposit. Phos. ether does nothing more—separated the deposit, which is dark and clots—the fluid over LXXXIX A was dark grey or slate and gave a feeble cone by sunlight and lens. Deposits put together—washed—dried—heated; evolved no phos.

or p. acid or other substance apparently, but was I believe metallic palladium. Dismissed.

15017. The Mercury and Phos., XC (15010). No mercury in solution—a dark deposit containing phosphorus, Mercury, etc. etc.; dismissed it.

15018. Mercury and deposit—does not seem to have gathered together. Agitation gave a fluid with blue tint. Added salt and agitated it. Is LXXXIV (15004); left. 19th. Mercury is taking up the gold gradually. 23rd. Still there is loose gold about.

15019. The deposits with camphine, LII A3 and LX*c* (15002). Poured off the camphine and put on absolute alcohol—the camphine brought away nothing. In the Alcohol the particles of LII A3 adhered strongly to the glass still—but in LX*c* they diffused through the fluid, giving it a dull blue transmitted colour. On the 19th in the same state. Alcohol off and water on: LII A3 still stuck to the glass; ammonia did not loosen them—in LX*c* gained nothing by the water—dismissed.

15020. As to the first tint produced. Some solution of gold (14946) in a bottle with a little Phos. ether—shaken together—pale red tint came on, which in dark tube shewed ruby. As it increased in depth it still shewed ruby—it was made XCII and left for further observation. Also some of the same sol. gold, with Phos. in Sul. Carb. *in great excess*, was put into a bottle, shaken and made XCI—its tint came on quicker, but it was also ruby. 19th. XCI was a dirty ruby colour and had a dull deposit—it was cleansed by cotton. XCII, the ether preparation: fluid almost entirely clear—deposit dark slate or amethystine—shaking up into a feeble dull blue tint—gold compressed. 23rd Sept. XCI: Amethystine—very little deposit—a poor cone by lens and sun, distinct but not dense—fine division but much absorption of light. XCII was a coarse settled deposit and was dismissed.

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15021. The old gold stained vellum has the ordinary purple stain to common observation but is of a beautiful ruby by transmitted light, when doubled and tripled even—it is an excellent localized divided gold. It is blue here and there, just as blued deposit in fluid form is. Lens light on to it gave strongly illuminated spot—

but the heat of a large lens soon shrivelled it up. Being soaked in distilled water to remove all soluble matter, it presented the same fine transmitted light as before. A portion being put into pure strong nitric acid did not change in tint—applied heat—the gelatine then disintegrated and dissolved, and the violet or amethystine particles flowed out in beautiful coloured streams. If urged further, these lost their colour as they would do by action of the acid—but if diluted, the dissolved gelatine re-coagulated and with the gold formed a coloured amethystine deposit which a filter took out.

15022. Moistened a piece of parchment with de la Rue solution and put it into the Sun shine (15040). Moistened a piece of vellum with own acid solution—it soon became somewhat amethystine. Put 3 drops of de la Rue solution into ½ fluid oz. of water and put a piece of vellum into it, leaving it there to soak (15039).

15023. Filtering out a diffused gold. LXXXIII (14998) is a fine ruby fluid with much diffused gold. Took the top portion and passed it through a simple paper filter—5 or 6 passages took out much gold, it being deposited on the first surface and in the paper—removed this filter, making it F LXXXIII 1, passed water through it, which removed none of the caught gold, and then dried it. Put the rest of the fluid several times through a new filter and so took out more gold, and made it F LXXXIII 2. Putting the fluid, now much reduced in gold, through a third filter, it took away very little each time. Adding salt to this fluid turned it from rosy to blue, and now the filtration soon separated the blue gold. Filter 1 had a rosy amethystine colour. Filter 2 took up the same tint, yet the liquid before passing the filter was *blue* in the dark tube; is the rosy tint of that in the paper due to the effect of reflection combined with the transmission?

15024. LV*a* A is an amethystine deposit (14912) in small quantity—amethystine in dark tube and would become ruby doubtless in depth. Put it through a paper filter—it would not catch well—passed through. Added a little salt but not much—filtered and now easier caught—gave an amethystine violet deposit—was dismissed.

15025. It is remarkable to see the ease with which the finer particles go through a paper filter well wetted. When these filters

are charged with gold, it is not so striking on the surface as when they are held up to the light. Much of the gold is *inside* of the paper.

15026. The deposit LVIII*b* (14899), which has been washed several times, had the water drawn off close—then poured off a thinner portion and left a thicker mud behind—both were light brown by reflected light and a fine deep blue by transmitted light. A spot of the thicker portion, being put on to the hand or a cloth or a card, lost its bright brown appearance and produced a dull purple or blue spot just like a gold stain—the gold became blue here. When dried on the hand, the spot had a purple brown appearance and a dull dead metallic lustre—it washed off, being in loose particles. Added its bulk of Glycerine to this thick and best portion—it immediately concentrated the gold particles—took away at once the beauty—made the gold clot and change from light brown to a dark red by reflexion, and from fine blue to dull feeble slate colour by transmission. Was now common condensed precipitated gold. Added water to bring it back if possible and made it LVIII*bb*—it did return in part and gave an improved transmitted blue.

15027. The other part of LVIII*b* (15026) was put into a tube to settle and then to be dried and heated.

15028. Sulphuretted hydrogen gas—LXXXIII fluid (15023) is in the dark tube violet or amethystine—sent in a little S. Hydrogen gas to atmosphere above and agitated—it became pure blue—yet when taken out and compared with the original, there was not much difference, so much effect is produced by the reflexion of the diffused particles. LXV (14944) was, in the dark tube, a fine clear ruby with few visible, i.e. larger particles present; the S. Hydrogen made it amethystine—seemed little changed when brought out into common light. LXI (14942) a beautifully clear ruby in the dark tube—the S. Hydrogen gave it a little tendency to amethyst, but less than in the former cases and less as the fluid is clearer, i.e. the particles diffused are smaller.

15029. Some of the solution of gold (14946) with Ether only, put into a bottle and shaken and left: made XCIII. 23rd Septr. Reduction as with glycerine LXXXV, or as with acid sul. iron—brown by reflected light—fine blue by transmitted light, not deep

in colour for gold appears to be in dense particles, there is so much light transmitted—a very good cone of rays by lens—merest trace of gold in solution.

15030. Set a dish with a solution of gold and floating particles of phosphorus to form films, green if possible.

15031. The deposit LVIII*c* (14930[1]) with water in dark tube is blue when dilute and blue when concentrated and strong.

15032. The glycerine and gold solution LXXXV (15005) is as before—it settles but slowly. The sun's rays and lens gives a good cone—the appearance is fine—the gold apparently brilliant as to reflexion. 23rd Septr.—is still the same—but settling.

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15033. The Ruby Vellum (15021) the old piece—part has been soaking in water for several days; there is no change in the tint. A portion of this being boiled in water for some hours made no change. 25th: very dark, but what rubbed up was unchanged and ruby. 29th: the same.

15034. A portion was put into a strong solution of salt—no change of the tint after 6 hours—left. 25th: no change. 29th: no change.

15035. A portion in dilute S.A.—no change in 6 hours. 25th: no change—29th: No change.

15036. A portion in strong pure Muriatic acid—curled it up—but after a time the vellum unrolled. After some hours the gold was gradually loosened and some came out into the acid, forming an amethystine fluid which gradually gave an amethystine deposit; the colour of that remaining in the vellum was unchanged. 25th: a colourless film left—and violet flocculi of gold and organic matter—a blue deposit on the glass. 29th: the same as on the 25th—dismissed.

15037. A portion in strong solution of potassa—after some hours, an uncoloured layer separated from the coloured part—the colour of the latter was unaltered. After 6 hours the colour of the solution was pale brown, probably from action on the vellum. 25th: Film same ruby colour as before—very soft and bruises

[1] ? 14929.

by pressure of the glass rod—the fluid is a little ruby or brown. 29th: just the same—tissue is a fine ruby.

15038. A portion in Strong Nitric acid—curled up at once—swelled and gradually disintegrated—the gold remained unchanged in the vellum—some became loose and, flowing into the acid, rendered it amethystine for a short time; but was soon changed by the acid, perhaps dissolved, as there may have been a little chloride present—the vellum disintegrates into fibres which float about and probably dissolve—as long as the gold is with them it remains amethystine. 25th: the disintegration is furthest here—what film is left is colourless—the gold is diffused in the Nitric acid and is blue, forming a good blue liquid. 29th: just as on the 25th, except that gold is deposited on the glass as a blue film—took out fragment of film—added a drop of brine to give chlorine and then the blue gold dissolved at once.

15039. The vellum in a weak neutral solution of gold (15022) four days ago has taken all the gold out of the solution. At the upper part where it was once wet, it has a fine ruby colour—but below that, where one wetting and soaking overlaps another, it is of a deep amethystine purple and even blue. In other parts it is merely pale brown or yellow, and yet the gold is in more abundance there and in quantity enough to give a dull metallic reflection. I think the gold has aggregated here into particles too large, and that above it is more perfectly separated.

15040. The piece of parchment with De la Rue moistening which was left in the window (15022) is stained superficially; by reflected light it is dark purple and metallic in points; by transmitted light it is blue and ruby in different parts, being ruby in the weakest parts. It is by no means so beautiful as the old piece.

15041. Paper rendered ruby or violet ruby by filtration of gold fluid (   ). Being looked through by dark tube so as to shut out lateral light, it had the same ruby or rosy hue as before—it is not due to reflexion, but the particles themselves may be considered as rosy.

15042. Portions of this paper were put into water, brine, dilute sulphuric acid, alcohol and camphine—the four first of these remained of like tint; water and brine caused no difference. Camphine gave its paper a tinge of blue, i.e. the tint passed a

little to blue and when the camphine was pressed out and dried off, this effect remained—it is slight but it is there.

15043. The dish with gold solution, etc. (15030) has a film of different thicknesses in different parts—it was taken off and placed on water to wash. Phosphorus in Ether being added to the pale reddish fluid shewed there was gold in solution, so it was put into a bottle and made XCIV. On the 25th it was passed through cotton to clear it of phosphorus films and was good ruby.

15043½. The film by reflexion was brilliant golden yellow—by transmission it was dark grey or lighter grey according to the thickness, but in some parts it was of a fine green; but these parts were few. When a part was placed on cyanide of potassium to dissolve it, gradually a remarkable change to green occurred—the paler grey parts became green first and afterwards, as they thinned, the thicker parts. This green was permanent, for when the altered film was washed and placed on glass to dry, the green remained.

15044. A part of this film, untouched by cyanide and attached to a glass plate, was heated to dull redness—it became as before time a thin gray film—a card moved all parts, so there was no attachment to the glass. Agate pressure did not give good greening effect on the thicker parts, but was good in greening on one thin uniform grey part—a green part became gray by the heat.

15045. A part which had become green by the action of the cyanide, being heated, lost its green tint soon—being removed from the heat and allowed to cool, the green did not return; the change was permanent; being heated more highly, i.e. up to dull red, and cooled—it did not adhere to the glass. Agate pressure here made many parts, especially the thinnest, of a beautiful green colour.

15046. Glycerine to gold mud, LVIII*bb* (15026). Has all deposited; poured off the fluid. The gold is now a common coarse deposit adhering to the bottom of the bottle, which shaken up with water gave a gross imperfect blue tint, obscure by reason of the many large particles. Dismissed. The other portion without glycerine settling in a tube, LVIII*b* (15027), has settled: quite clear for half an inch at the top—then there is an amethyst coloured top to the brown cloud beneath—left to settle more perfectly (15062).

15047. The gold preparation LXXXI (15011) decanted—the sediment, being blue in the dark tube, was put to LXXXI A and the decanted fluid replaced as LXXXI to settle more.

15047½. The gold preparation LXXXIII (15012) is a beautiful fluid—half an inch at the top nearly clear and very rosy—the rest turbid—but common day light could pierce the whole, making it appear ruby or amethystine. Being decanted, the fluid was left as LXXXIII. The part left was divided into two parts, the more fluid being poured off and made LXXXIII D; the less fluid part was blue by transmitted light and was added to LXXXIII B. LXXXIII C deposit (15012) is a clear ruby fluid with settling clouds and a beautiful ruby deposit—it was left undisturbed.

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15048. Two days ago, I took two portions of the weak gold solution (14946), made one a little acid by Hydrochloric acid and left the other neutral, and dipped like pieces of fine vellum into each, leaving them in soak. To-day the neutral solution shews no sign of gold on the vellum except perhaps a very faint yellow tint; yet the gold is very nearly all out of the solution. In the acid solution, the vellum was more thoroughly soaked and wet—it was coloured blue and amethyst here and there irregularly, half its surface at least not being coloured at all—the fluid contained gold particles and had a feeble bluish deposit at the bottom. There is still a little gold in solution, as much as was in the former glass. I hung both these pieces up in the air to dry.

The vellum is dressed with chalk, etc.—it should be soaked in a little dilute Mur. acid before application of the gold.

15049. I put on two like solutions, 14946, neutral and acid, with like vellum in to repeat the result. 29th. The results were just as before (15048), the *neutral* not stained—the *acid* stained irregularly. The neutral yet contains a little gold dissolved—the acid solution none. Sol. of Gold is too weak also.

15050. Moistened the surface of vellum in places with de la Rue strong solution of gold and also with a weak acid solution—the former produced only an irregular stain, yellow and violet—mottled—golden by reflexion in parts. The weak acid solution has produced a fine ruby stain.

15051. As to progress of change in colour by concentration (15000). No. LXXXIII C is a beautiful ruby deposit with a ruby fluid over it–i.e. the deposit is ruby at the edges of the settlement where only it can be seen through. First decanted off the fluid–then poured off the more mobile portion of settlement, leaving the lower heavier part–but much mixture of the parts occurred. The lower heavier part was blue in the dark tube with all degrees of intensity. The second was amethystine and by darkening came to blue; examine this again. The fluid was ruby with all depths. Seems to be difficult to get a clear ruby deposit–always ruby (15059).

15052. The deposit LXXXIII D ( ) in like manner divided into fluid, middle fluid and bottom deposit. The fluid is very turbid–amethystine and in the dark tube *rises to ruby*–the middle fluid is blue amethystine and rises by increase of matter *to ruby*. The deposit is blue. Put together again.

15053. A voltaic battery of 5 pr. of Grove's plates with platina terminations (wires) was applied to some gold fluids (14719). Results just as before were obtained with XCIV, an amethystine fluid–LXVII, a ruby fluid and LXXXIII C, a fine deposit. The spark of the battery occurred beneath the fluids, so free from salts, etc. were they, when the wire ends were brought together. The action was not continued for any time.

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15054. Soaked some fine vellum in dilute hydrochloric acid, which effervesced a little, and wetted it thoroughly–then poured off the fluid–shook off what I could and added a little of the shelf solution of gold, which is acid, to soak it. After a time, left it in ordinary day light at a window to act. 7th Octr. No good result in this or the other varied cases–parts were stained ruby and violet but uncertainly–the vellum seems to have little power of reducing the gold to the ruby or violet state.

15055. Soaked a piece of foolscap in dilute hydrochloric acid–poured off the acid–moistd. the paper with sol. of gold (that on the shelf) and left it moist in the tube in the window for the action of day light. 7th Octr. Even after so many days, no good reducing effect–dismissed.

15056. On the 25th I put some of the soaked ruby vellum (15021) into an aqueous solution of chlorine—there was no sensible immediate action—it was left. To-day, it appears that the gold has been gradually attacked and dissolved at the edges and at places in the middle, so that though central ruby gold remains, it is thinned and in places gone. The free chlorine is all gone—else no doubt the whole of the gold would have been dissolved.

15057. A reflected sun beam was sent horizontally—then passed across a glass cell containing $1\frac{1}{2}$ inches in thickness of a solution of sulphate of Quinia—then through another glass cell of equal size containing either water or some of the gold fluids—and then into another solution of sulphate of Quinia contained in a blackened glass with a clear place in the side for the entrance of the ray; the object being to ascertain whether a ray deprived of Stokes' rays by the first cell would be affected differently if passed through the gold fluid and through mere water. There was no sign of a restoration of the Stokes' power. On the other hand, when the first cell was removed and the direct sun ray passed through gold fluids and water—the former appeared to take out the Stokes' ray far more rapidly than water. Still, allowance has to be made for the colour. XLII, a fair clear ruby fluid—LXIV, a pale amethyst fluid—LXVII, a deep ruby fluid, clear—and XCIV, a good clear ruby fluid—were employed in these experiments.

15058. Looked at the Voltaic spark of 10 pr. of plates through all the above media (15057) and at the sun's image reflected from a small silved. sphere, but obtained no particular result of the action of particles. Also looked at the voltaic spark through certain phosphorus films of gold, namely Numbers 8, 9, 11, 12, 15, 19, 23, 36, 37, 38, 40 and 42. No particular result.

15059. As to progress of change in colour (15051) and LXXXIII C, I now again divided it into three portions by pouring off as before. The first fluid was amethyst and increased up to fine ruby by deepng. the tint. The second was only in small quantity, not well enough for my experiment. When in little, it was amethyst, and I think if I had had enough, would have risen up to *Ruby*. The third or deposit part was blue. A very little of this deposit added to the second when at its deepest made it blue as a whole—but then when this was diluted or thinned, it did not return to

amethyst but continued blue, according to what seems the general progression. Again, LXXXIII D (15052) as a whole was a beautiful amethyst or ruby violet fluid. Being poured off into three portions: the first fluid rose up by condensation to a glorious ruby—the second fluid portion was amethystine and kept so through much concentration; perhaps at very last just before it was dark it tended to ruby. The third part or deposit was blue with all states of concentration.

15060. LXXXI. Ruby (15047)—decanted—the finer part of the deposit was amethystine and in the dark tube rose when almost obscure to ruby; it was made LXXXI B. The coarse part was made LXXXI A, which as a whole is always blue.

15061. LXXXVI A deposit (   ) had the fluid poured off—the mud deposit was blue to day light—it was put on to a glass plate, floating on it, and the electric spark was looked at through it—there was nothing particular. Thought it was sometimes red, but then the spark was dull and the carbons red at these moments.

15062. To-day, the tube LVIII *b* (15046) has settled so well that I have poured off the fluid and left the tube with its wet contents in association with quick lime under cover to dry it thoroughly. 7th Octr.—dry. Golden—by transmitted light black—or in very thin places, gray—or amethystine even—touched by glass rod, the gold clings together—perfectly neutral on damp litmus paper—no P.A.—fibres present, most likely of mouldiness. Heated—gold became more bright and of a pale brown colour—no water set free—no acid formed—the gray part became ruby or amethystine—change the same in nature as when film or gold deposits are heated.

15063. XLII (14910) has had some more half used solution of gold and phos. ether added to it.

15064. Examined some gold leaves for the red marks. I incline to think these places only look red by contrast with the surrounding green part.

15065. Put a weak solution of De la Rue gold into a large Wedgwood's dish yesterday evng. and on it fine little particles of phosphorus, some from evap. of sol. of phosphorus in S. of C. and some from cutting of solid phosphorus. All formed films of like character–grey over the general surface and brilliant golden round the particles of phosphorus. These were of such thickness, etc. as to be remarkable for their green transmitted light (made 406 to 410), but the thinner part was gray, etc. On solution of cyanide of potassium–there was not much change to green in the grey parts, yet a tendency was evident. Also in solution of chlorine there was a change, but it was *clearly towards* amethystine or ruby tint.

15066. No ruby fluid had been formed by this floating phosphorus, which was in very small pieces–nearly all the gold was separated, for very little remained to be taken out by Phosphorized ether. This and some previous fluid were put into two bottle[s] as Miscellaneous fluid and marked XCV and XCVI.

15067. The vellum, thick and thin, moistened in stronger and weaker solutions of chloride of gold, acid or not–then put into atmospheres containing ether vapour–or phosphorus vapour–or hydrogen–or the vellums touched with a little sugar–or between poles of a voltaic battery–but no useful result. Gold reduced probably but often in films with dull metallic reflection and not presenting fine transmitted colour–the particles not being in the favourably divided state.

15068. Think I remember that my excellent piece was not vellum but a piece of gut. So procured a gut and a bladder–and soaked them in water; then cut off slips, laid them on glass plates, one piece with one side upwds. and the other piece with the other side up. Moistened them in four different places with solution of gold, so as to have 16 specimens, placed them in a plate and covered them with glass dishes and left them exposed to ordinary

day light in my room. I left also a piece of the gut in a tube with a little of our shelf solution of gold, corked up and exposed in the same light—the specimens were marked thus:*

1. De la Rue's strong neutral solution.
2. Do. . . . . much weakened by water.
3. Our shelf acid solution.
4. Do. . . . weakened by water.

Traces of staining and rubification soon appeared on the gut.

9TH OCTR.

15069. The gut specimens are by far the best—the bladder specimens are very poor by comparison, but of the same nature. In all the cases the places touched by strong solution 1 are the best—2 places are next—3 is feeble in effect even on the gut—4 hardly sensible. No. 1 on the gut is a very fine deep ruby stain by transmitted light—just like the old vellum as I called it—the gut rather—so the effect is reproduceable at pleasure.

15070. The piece of gut in the tube (15069[1]) with shelf solution of gold is as fine as Gut No. 1 stain, perhaps even finer. The fluid that is left is a little violet in colour but contains *no gold* in solution. All the gold has been reduced within the pores of the membrane—the stain is progressive from a part hardly coloured to a part perfectly opaque and black, but wherever light passes the tint is fine ruby.

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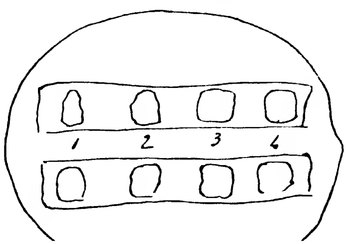
15071. The bottle LXVI (14889) with five different specimens of the same ruby fluid to dry—looked at now after 6 weeks of dessiccation. One tube was quite dry and had left a blue deposit. Two other of the tubes were not dry but the gold had settled on the glass—dark blue deposit. The other two specimens had thrown down some deposit, but were still amethystine—left over lime to dry. It is evident that as the fluid dimnshd. the gold came together—it is also evident that the gold changed in all from ruby to blue.

[1] Should be 15068.

* [15068]



15072. Took a piece of the gut (15068), washed it quickly and put into a tube, covering it entirely with a weak solution of gold from the shelf, to see how the general soaking, etc. would do–left it in ordinary light on the filter stand. 13th. The gut a good ruby–the fluid a little ruby–poured off–gold all out of solution–salt added to fluid did not make it blue; remained ruby–flocculi appeared which settled or filtered out and were ruby–gold and gelatine–looks like an association of the two. The gut was soaked in water–a surface film or membrane stripped off–it was ruby; so was the other part, but more on surface than the middle–some part of the middle not stained–not reached by the gold–is a fine specimen of ruby membrane (15075).

15073. Took the green film 410 (15065) and heated it to fair redness on the glass. It was a thick circular gold film, with thinner grey film about it on the outside. As the heat rose, the gold lost in reflective power–the green transmitted ray disappeared and the film looked grey, and as if a contracted fissured film from adhesion and retraction of the particles of gold laterally–the thin surrndg. gray part became ruby or violet ruby by the heat. The rub of a card edge removed the gold from the glass both in the thicker and thinner parts. *Agate pressure* applied to the thin part developed most beautiful green and reflective power–finest I have seen; applied to the thick film it did the same, except that there was too much gold and so the place was dark green, almost black. The agate slipped and in slipping did not remove the gold but burnished a line, producing fine yellow reflexion and a dark green transmission. On the whole, this gold seems in a good state for producing the pressure effect. The unheated films 406, etc. when pressed came off, and results could not be obtained.

15074. A ray of light was polarized by a Nichol's prism in a vertical plane and extinguished by a second Nichol's prism employed as analyzer. Whilst in this state, a plate of glass introduced across the polarized ray so as to be at right angles to it, did nothing, i.e. did not depolarize the ray. If inclined so as to make an angle of 30° or 45° or thereabout with the polarized ray, it rendered the image of the lamp flame visible. If the inclination was either in the plane of polarization or at right angles to the plane of polarization, the depolarizing effect, so to call it, was the least, being nul;

but if it was in the intermediate angles, i.e. at 45° with the plane of polarization on either side, it was a maximum. This I suppose is an effect like that of Fresnel's rhomb and common to all transparent bodies not crystalline and therefore not depolarizing as crystals depolarize. Well, Gold leaf placed over an aperture does the same thing—but I do not think this proves it to be a depolarizing body, more than glass, though it proves it to be transparent (15085).

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15075. Put on another piece of the gut (15072) into a solution in a tube, 8 drops of shelf solution being employed—the change began at once slowly. Next day, all the gold was out of solution and the fluid was colourless, not ruby. The vellum was ruby in parts but where in rolling up it had touched and excluded the circulation of the solution, there it was not ruby; also the bottom of the roll in the tube was not ruby, the top being the place of the gold. Effect is superficial here also in many parts.

15076. Three gelatines selected and jellys prepared from them by solution in water. Nos. I and III give good clear jellies; No. II opaque and so not to be used.

No. I jelly in large watch glass and 2 drops of shelf gold (acid) added: made a coagulum which on stirring in disappeared much, but a part left, this due to the gold solution—the part was removed by the stirrer, and the mixture, from which the gold did not come down at once, was left to cool, coagulate and act exposed to common day light—is A, 411. Next day, very little change—jelly coagulated—there are blue clots and striæ but the general mass is colourless. 17th Octr.—jelly is as before—drying up—leaving pale blue coat on the glass, and no metallic lustre.

15077. No. I jelly and 2 drops de la Rue solution—strong coagulum, which did not dissolve on stirring, but much came away on the rod—nevertheless some was mingled in the solution and it was left to cool—and act—is B, I think 412. Next day, has a metallic reflexion on the surface and a little blue striæ within—amethystine round the edge and a peculiar amethystine reflexion when the concave side of the glass is upwards, but the fluid is not coloured either ruby or blue as a mass. 17th Octr.—has a

brilliant golden reflexion on upper surface and transmits a blue light; but effect is entirely superficial, for a touch with the finger removes the surface and the part beneath is colourless—is drying up—this result is just a film of gold like the phos. films. A drop of the same jelly on a glass plate is now dry and shews exactly the same appearance—but the gold is entirely superficial and scrapes off, leaving the part beneath colourless—is this an air voltaic effect?

15078. No. I jelly. 1 drop of de la Rue previously diluted—stirred together and left. Made C, 413. Next day, no sensible effect as yet. 17th Octr.—drying up—no good gold effect, only a little accidental blue in patches.

15079. D is No. III. Jelly alone 414.

No. III jelly, with shelf gold previously diluted—mixed fairly—left—it began to change to blue in less than an hour. Made E, 415. Next day—it was of a fine ruby colour throughout—by ray and lens shewed the cone and was in fact a fine ruby fluid solidified. 17th Octr. Very fine ruby—is deepening in colour—the dry part is violet and blue by contrast with the middle—no superficial film of gold.

15080. No. III jelly, with de la Rue gold previously diluted and mixed—left as F, 416. Next day, was uniformly affected, brown by reflected light, blue by transmitted light and shewed the cone of rays; this a good case of solidified blue fluid, but will probably become deeper in colour. 17th Octr.—is a faint blue—no gold film on surface.

15081. When the gold seems to mix in with the jelly, it alters it far more than the same addition of water (cold) does—it makes it glairy and stringy. Pure M. Acid is as water in that respect, not as gold.

15082. 5 drops of de la Rue solution in water divided into two bottles: into one, XCVIII, no salt was added; into the other, XCVII, 14 drops of brine were added. Then a weak solution of jelly was made and equal quantities of it, being small quantities, added to the bottles to see what the reducing effect of jelly is. Next day, no sensible effect in either—added more jelly to both, the quantities being still small—agitated and left. No sensible action on the 15th Octr. 17th Octr.—no sensible action in either

XCVII or XCVIII, yet found gold in both by proto chlo. tin. Put a part of each into separate bottles—added Phosphorus in ether—action at once in both, and XCVIII quickest and best-marked XCVII P, etc. 18th Octr.—XCVII P, deposit in flocculi, dull—reduction of gold such as to give a poor fluid for colour. XCVIII P, a fair ruby fluid but with some deposit. So Jelly alone does not easily reduce weak solution of gold. 23rd Octr. XCVII and XCVIII. Still colourless—but those with phosphorus had now deposited—dismissed all four.

15083. Two drops of shelf solution of gold into small quantity of water—this mixed by strong agitation with an equal vol. of Jelly No. 1 (15077[1]) in a tube—being warmed a little and left to cool, it then gelatinized well—was made XCIX and left in sun light. Next day—no sensible effect. 17th Octr.—no sensible action. Melted it and added a little Phos. in S.C., agitated well and left.

15084. Repeated arrangement (15074) of polarized ray and tried varying objects and substances.

A thick plate of annealed glass produced all the effects.

A cube of glass, depolarizing in places, produced the same effect in the black cross parts.

Very thin Microscopic glass—the same effect excellently.

A fresh soap sud film on a copper ring—same effect.

A dried soap film Do. . . . . —the same effect.

Film of Collodion—excellent effect, the same.

A film of resin on a copper ring—same effect.

Sheets of Jelly depolarized in parts—but wetting them, this was reduced, and then the peculiar effect sought for came out well.

A film of Jelly—in transparent gauze fabric—shewed the effect exceedingly well.

15085. When a ray of light impinges at the proper angle on a plate of glass, the reflected ray is altogether polarized; that which passes through contains also polarized light, but the proportion of common light mixed with it is so great that an analyzer in common day time does not shew much difference. When a piece of gold leaf is used for the glass, and the angle it makes with the ray is considerable, the transmitted ray contains much polarized

[1] ? 15076.

light and the analyser in revolving easily shews the difference (15074). When the angle between the incident ray and the gold leaf is about 15°, nearly the whole of the transmitted light is polarized.

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15086. Prepared some portions of dense gold precipitate for the experiment at the magnet. Took LVIII*c* deposit (15031), decanted off the fluid and had an excellent dence preparation left. Examined LXII (14917, 33)—drew off the supernatent liquid and dismissed it—the mud will be an excellent preparation for the experiment. Went to LXXXI B (15060): it is an excellent deposit from ether phosphorus and will serve for a result. Took the different preparations of LXXXIII, drew off the fluids and made them the LXXXIII, dividing the deposits into two portions, one transmitting pure blue, which was made LXXXIII D, and the other transmitting a ruby or amethyst tint, which was made LXXXIII C; both these will do for the magnetic experiment.

15087. 18 drops of Shelf gold diluted—halved—a little hydrochloric acid added to one half and then its bulk of the warm mixed jellies I and III (15076) added—all well stirred up, made C and left. On the morrow it had gelatinized, weakly rather, and the gold was reduced, but as by acid sulphate of iron—for it looked yellow brown by reflected light and feeble blue by transmitted light; and the gold was deposited in large striæ, as if very imperfectly mixed. 17th Octr.—is in same state.

15088. The other half of the gold had 18 or 20 drops of pure brine added to [it] and then an equal or larger volume of the jellies I and III. It was made CI and left. On the morrow it was a fine ruby throughout by transmitted light—is apparently an uniform mixture of gold—shews the cone as the other does by the lens and sun's ray—is stiffer than the other and a very good result. 17th Octr.—is deepening in its ruby tint and more opaque to reflected light.

16TH OCTR. 1856.

15089. Expts. on polarized light and polarization. For the purpose of repeating the experiments (15074) on the depolarizing

influence of inclined glass, gold leaf, etc. in other media than air, I employed a rectangular glass trough, and putting water or camphine into it, then introduced the glass or gold plate, inclining it at various angles as needed. As the ray of light employed was polarized in a plane inclined 45° to the perpendicular—so the varying inclination of the plates was made about a perpendicular axis, thus producing the maximum effect of depolarization.

First with the polarized ray and air medium—these results came out:

15090. A Glass plate (crown)—good depolarizing effect as before (15074).

15091. Gold leaf on card or copper hole—excellent as before and in same direction as the glass, i.e. like inclination produced like kinds of effect (15074).

15092. Film No. 8 ( ) acted as glass and gold leaf—this rather unexpected, as it cannot be considered as continuous; but in such a case it must have an *effective optical continuity*. Its effect was distinguished from that of the glass which supported it by passing the ray through it and through side places where there was glass only and no film.

15093. Film No. 8 produced another effect. When perpendicular to the polarized beam, the thicker part near one end acted on the ray and rendered the image of the light visible and red—in a manner not occuring with the lower thinner part—nor with the clear glass of the plate holding it—nor with the glass in the place where the film acted if the film was cleared away. This is like a *crystalline effect*. No. 41 ( ), consisting of several superposed films, and obstructing common light a good deal, did not produce any general effect of the kind except in one or two spots—there the result was clear, but the places small. Film 9 did not shew it. Film 14 did not shew it. Film 96 has been heated and so the glass interferes, being in a constrained condition. Much care is required not to hold the plates in the warm fingers—or else there effect might be mistaken for that of the film.

15094. Gold leaf does not shew this effect.

15095. The effect in film No. 8 is quite clear and not dependant on any warming of the glass. It is not an effect of rotation, for the red tint produced has no corresponding blue by any motion

of the analyzer; and the most obscure position of the analyzer for it is also the most obscure position for the clear glass.

15096. Proceeded to work with media and the polarized ray, using first the *glass cell and water* (15089). Now the glass cell itself depolarized through deficient annealing, but I chose out a black place where the effect was nul, and then examined a plate of crown glass and also gold leaf stretched over a slit in a copper plate, at that place. The oblique plate of glass acted as in air but I think with less effect—like inclination produced a like, though not an equal, result.

15097. Gold leaf in the water—acted generally as the glass—and therefore corresponded to the effects in air.

15097½. Proceeded to employ *camphine* as the medium (15089), hoping to reduce the power of the inclined crown glass plate and so distinguish that of the gold leaf or films held upon it. In this case the cell employed did not seem to interfere much, but the camphine rotated the ray. So adjusted the analyser as to give the least luminous effect between the red and the blue—then introduced a plate of glass. When transverse, the effect was nothing—when oblique, it was very small, for it did not increase the sensible intensity of the lamp image though it changed its tint a little.

15098. But when gold leaf was on the glass, the effect was very different and very interesting. Whilst vertical to the ray, it stopped the remains of light, and in that position the glass of the plate transferred onwds. a good image through the analyser—but when inclined, the image through the gold became quite bright, colour almost entirely disappearing. It was strange to see the gold leaf becoming quite luminous by inclination—and it was still stranger to see that, by moving the inclined plate up and down, the gold was far more transparent to the polarized beam than the glass. The absolute effect of gold leaf as a transparent plate is well shewn by the experiment, and also the high refractive power of gold—and the bearings of this relative force in the cases of air, camphine, Glass and Gold.

15099. Could now proceed to examine the effects of films which before were untouchable in air because I had nothing but glass to support them. Film No. 14 produced the same effect as gold

leaf, *as well or better*. On removing part of the film in the middle, that part as glass only produced no sensible effect. Film No. 9—produces the same excellent effect. The colour of the image in the analyzer is at first a very feeble reddish blue—when the oblique film is in place, it rises up to a comparitively bright yellow white. I tried film 17, which has been heated to 96, but the tension state of the glass was such as to prevent me from obtaining a result.

15100. Now as these films produce the effect of an inclined plate of gold or glass—they must be in effect optically continuous; i.e., the form of them as a layer causes them to act, which it would not do if the two consecutive particles had not a joint action on a light vibration.

15101. Film No. 8, produced the same effect (15099). I examined it also as to the effect produced when it was perpendicular to the polarized ray (15093)—the effect was the same; if the image in the polarizer was at first of a feeble blue, the gold film converted it into red.

15102. A glass plate dipped in camphine and held vertical to the ray does not carry enough fluid to shew any rotating effect.

15103. Now worked with an *unpolarized ray in air*, endeavouring to polarize by the plates, films, etc. and examine by the analyzer.

15104. Gold leaf over hole in a copper plate polarizes when inclined as before (   ). Gold leaf laid on a glass plate by water can have its polarizing effect distinguished—for as the glass when inclined gives a bright image in all positions of the analyzer because of the quantity of unpolarized light which goes through, the gold leaf changes this; for acting on the light which has passed or would pass the glass, it polarizes it and the rotation of the analyzer shews the full effect.

15105. The film No. 8 on its glass inclined in air polarized in like manner as gold leaf, but it required great inclination and then the ray was only partially polarized, not completely.

15106. The films superposed, No. 41, produced a like effect but I think not a better. Even at greatest inclination, when the analyzer is rotated, a little light passes at the minimum and not much at the maximum.

15107. Heated film, No. 408. The thinner ruby part gave changes of colour as the analyzer was turned, which the central thicker

part of the film did not, so that they are probably dependant on the film, but I did not clear it away to ascertain the effect of the inclined glass alone—the glass being in tension teased me.

15108. The heated films 406 and 408 shew excellent effects of agate pressure—the thinner uniform parts become beautifully green, being before by the heat a good amethyst or ruby—the thicker central parts looking like dead gold, by the pressure became golden by reflection and almost black, a green black, by transmission.

15109. So the films polarize as gold leaf does, but in variable degrees and not so finely as gold leaf.

15110. Repeated some of the foregoing experiments with an unpolarized ray, employing *camphine* as the media about the plate to get rid of the glass effect. *Gold leaf on glass*—its effect in polarizing very distinct.

15111. Film No. 9—on glass oblique in camphine. As the analyzer was rotated, there were changes of colour from red to green and blue that do not occur on the glass itself—not great changes but very distinct—shewing the real effect of the films.

15112. Film 14. Same effect as with No. 9. Here a part of the glass in the middle of the film has been cleared, so that it was easy to see the film produced its own effect and not the glass beneath it. Film No. 8 produced like effects.

15113. Film 96 has been heated—there are changes as the analyzer is revolved, but I did not separate them from the effects of the heated glass.

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15114. A. 5 drops de la Rue's solution in $2\frac{1}{2}$ measures of water.
B. Do. . . . . with $\frac{1}{2}$ measure of brine.
C. 15 drops shelf solution in 3 measures of water.
D. Do. . . . . with measure of brine.
E. Strong jelly of No. 1 gelatine—alone, but warmed to melt it.
F. Do. . . . . with good deal brine.
G. Do. . . . . with 2 drops of P. in S. C. shaken up in bottle.

1 Equal measures of A and E, i.e. de la Rue and Jelly.
2 . . . . A and G, i.e. de la Rue and Jelly with phosphorus.
3 . . . . B and F, i.e. de la Rue salted and jelly salted.
4 . . . . B and G, i.e. de la Rue salted and Jelly with phosphorus.
5 . . . . C and E, i.e. shelf solution and Jelly.
6 . . . . C and G, i.e. shelf solution and jelly with phosphorus.
7 . . . . D and E, i.e. shelf solution salted and jelly.
8 . . . . D and G, i.e. shelf solution and Jelly with phosphorus.
9 . . . . D and E, i.e. shelf solution and jelly with brine.
10 2 vols. G and 1 vol. C and D, i.e. shelf solution, some salt and jelly with phosphorus.

All these were mixed warm and with agitation in bottles and then left to jelly. Nos. 2, 6, 8 and 10 began to change at once.

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15115. The Jellies (15114). All gelatinized, but those with phosphorus the least. See 15206.

1—not altered.
2—very deep ruby.
3—not altered.
4—feeble ruby.
5—not altered.
6—moderate ruby.
7—clear ruby.
8—clear and deeper ruby.
9—blue and turbid like acid Sul. Iron effect.
10—Red brown.

15116. Worked with a polarized ray in air as the surrounding medium (15090), interposing the gold leaf, glass plate or film on it obliquely as before. Now the following leaves, namely, extra deep gold—deep gold—and new gold gave the depolarizing result. Pale gold and white gold gave effects hardly sensible: they were too thick. Deep gold thinned by cyanide was very good. So also was a variable film of gold on a large glass plate—but the

effect of the glass in air mingles up with these effects, and when a film is employed which transmits much common light, it is not easy to distinguish its effect from that of the glass ( ).

15117. Sought after the peculiar effect of Film No. 8 (15093). It was there again very distinctly. But with other specimens, as Nos. 111, 259, 260, 284, 309, 312, 368, 369, 372, 375, and also 51 containing many superposed films, there was no effect. The thinned gold, 68—the deflagration of gold and silver, 319, 345, did nothing—except at middle line of 345, where it had been heated by the discharge. The silver film 49 and the palladium film 44 did nothing of the kind. I am still doubtful about the case of No. 8 and can only settle it by wiping the gold off and then examining the glass beneath.

15118. Now introduced *camphine* as the medium, using still the *polarized ray*. First put the cell into the polarized ray; it was so well annealed as to depolarize only a little here and there, and places quite free from that effect were found and used. Then introduced the camphine; rotation of the ray and the colours appeared—adjusted the analyzer for the purple intermediate between the blue and red, which is also the least illumination position.

15119. Then a plate of crown glass into the camphine, vertical to the ray: it changed the purple slightly toward red, and it had the same effect when inclined on either side, but in a somewhat greater degree. This is the glass effect—is very small and is easily distinguished from the effects to be observed.

15120. For when the camphine is away and air is the medium and the analyzer in position to extinguish the ray—putting in the glass obliquely brings in the image—and it requires a little direct rotation of the analyzer to reduce this image to the minimum effect; then removing the glass, the image is left bright, and it requires reverse rotation of the analyzer to extinguish it. But when the image is extinguished in air, if the camphine be introduced, it requires direct rotation of the analyzer to obtain minimum image; if then the glass be put in obliquely, the effect is very small but the analyzer has to move a very little *reverse*, and when the glass is taken out, it has to be rotated *direct* a very little, so as to restore the minimum state. So inclined glass in air requires rotation one way but in camphine it requires rotation the other

way—for the glass seems intermediate yet nearer camphine than air.

15121. Gold leaf on crown glass, the same as that employed before (15098). When transverse, no effect. When oblique, a fine effect, the image of the flame appearing not merely coloured but a bright white. Rotation of the analyzer could reduce this brightness, though not to darkness, but a minimum, often of considerable luminosity and between the blue and yellow orange tints; but it required *direct* rotation of the analyzer to produce this effect, and when gold leaf taken out, then reverse rotation of the analyzer brought back the minimum image. So Gold and glass are contrasted here in camphine and before the gold can shew its effect, it has to compensate for that due to the glass which supports it; in fact, only the difference of the powers of gold and glass appear here.

15122. Does not this shew that the optic force (query refractive power) of the camphine is between those of glass and gold? Also when glass and gold polarize a ray by transmission, both being in camphine, should the two rays, both being polarized in the same plane, be alike or will some difference come out, perhaps made manifest by rotation phenomena?

15123. A second gold leaf on glass shewed exactly the same phenomena.

15124. It is of course understood that in these experiments the ray of light is polarized in a plane inclined 45° to the vertical and the plates incline about a vertical axis.

15125. When delicate experiments are in hand, it is good to hold the glass plates by corks, so as to avoid changing their temperature in places.

15126. The thinned gold leaves Nos. 68, 69, 70, 71 and 73, all produced well the same effect as gold. Some seemed to require more *direct* rotation of the analyzer than others to produce a minimum; such was 73; 71 seemed to require more and so also 70, etc. Is it the thickest that requires most, for Gold leaf needs it?

Polarized ray—camphine medium—inclined films and plates.

15127. The gold films Nos. 8, 9, 14, 111, 205, 259, 260, 309, 369, 372, 375 all produced the effect in a fine degree, those that were

uniform and of a good gray tint, as 259, 260, 372, 375, giving the best image. All required direct rotation of the analyzer to reduce the image to a minimum. These films did not seem to require so much as gold leaf and generally the thinner films seemed to require least. Probably the films and thinned gold leaf may be considered as porous and letting through some of the light as affected by the glass, and as this is affected by the rotation in a contrary direction, it may lessen the effect of that light affected by the gold and even neutralize it, as appears to be the case in some of the instances. One of these films, No. 14, had a part cleared away from the middle—it was most interesting to contrast the effect at that place with the effect of the film.

15128. No. 41 consists of 4 superposed films—No. 51 of 13 superposed. Both acted as simple films, except where the light was too much obscured. The part of 51 containing fewest films acted best. Super-position merely seems to give confusion or obstruction. Single films appear to have a regularity, a lateral continuity about them, which makes them approach to thin continuous plates.

15129. Nos. 284, 312, 368, 380, 383 are gold films of very variable thickness. All parts acted as before, even the thick golden rings, but the thin parts acted as well quite as the thick parts.

15130. No. 406—film heated—the glass has been so left by the heat that all particular results are too much hidden.

15131. Now proceeded to examine the deposits from deflagrations of gold: Nos. 319, 322, 386, 394, 400. All of them acted just as gold, requiring the same rotation of the analyzer and with various degrees of force in the different parts of the deposits. This is a very interesting result as it removes the gold still further than before from the condition of thin plates to that of scattered particles.

15132. *Platinum deposit*, No. 85. No sensible effect—doubt if there is any deposit on the glass—do not see it.

15133. *Palladium film*, Nos. 43, 44, 45, 46. All these produced a most excellent effect—like that of the gold leaf. The analyzer required direct rotation, as with gold, to compensate the effect. No. 46 gave such an image that the right hand rotation of the analyzer nearly extinguished the image. I think these films are very continuous—they give an excellent reflexion even in the camphine.

15134. *Silver films*, Nos. 47 and 49. Same effect as gold and palladium—very good. Nos. 342, 345, 348 are silver deflagrations. They also act well—but require very little rotation of the analyzer. 347 was a heated result and therefore could shew nothing here.

15135. *Colour of gold deposits.* Have a little cell with flat sides, also a shining sun. Deposit LXII in the cell—then looked with eye close up to the cell at cloud light—it was blue, scarcely visible—looked at the sun direct—it also blue—thickened the deposit until sun only just visible—it was still blue but amethystine—did not pass to ruby. Deposit LVIII C tried in the same way required dilution to render the sun visible; then it was amethystine and so was the colour when the liquid was still more dilute. Deposit LXXXI B was blue by sky observation—by light of the sun violet; and when sun focus sent on to and through it on to a card, light was amethystine tending to ruby. LXXXIII D deposit was dense; in diluting, I added water at the top so as to get a screen of varying depth of gold from top to bottom. Could see the sun well at the top—not at all at the bottom—but the tint was the same in all parts—no change from ruby to blue. Deposit LXXXIII C arranged in the same manner. It was a glorious ruby in all parts—very luminous at the top, opaque at the bottom.

15136. *Experiments in different media.* The little lamp—Nicol tube polarizer—glass cell—and Nicol's prism analyzer arranged as before (15089). Analyzer adjusted to darkness. *Sulphide of carbon* put into the cell—no effect, i.e. no image—*no rotation*—no displacement—so a good fluid in that respect. The glass plate was put in vertical—no change—then it was gradually inclined, but it did nothing until a certain moment when a spectrum, i.e. a blue, green and red image appeared which, by the least further motion, disappeared altogether. When the analyzer was turned so as to let light through, there was an analogous effect; i.e. as the glass was inclined, the light continued the same until a spectrum appeared and immediately after disappeared because of total reflexion. With a plane unpolarized ray the results were

So this fluid will do with glass as a supporter for films, dusts, etc.

15137. When the glass was put in direct, i.e. transverse, it appeared to produce a very little effect, bringing in an image, and

it required a little direct rotation of the analyzer to put it out—further rotation produced a feeble orange tint, after which of course brightening of the image; if after the glass was put in transverse the analyzer rotation was made reverse, the image cleared a little and brightened at once; had the ray been pure white light the reverse rotation would probably have shewn blue at the beginning. Must examine this result again. See 15158 and onwards, also 15213.

15138. Gold leaf on glass in this S. of Carbon. When vertical to the ray—it did nothing—when oblique it depolarized—it then required direct rotation of the analyzer to reduce the image to a minimum, which was still a coloured image as in camphine. So is it not a rotation of the plane of polarization by the gold leaf, as camphine rotation is here away? If but *little* inclined, the blue and red come on as the analyzer revolves but the minimum is bright—if much inclined, less light passes but the extinction by revolution of the analyzer is better. A second gold leaf on glass shewed the same thing. No. 71, which is gold thinned, also shewed the same effects.

15139. Again gold leaf—oblique—direct rotation of the analyzer gradually diminishes the light and then brings in blue and so on to brightness. Reverse rotation brings in warm tint; have to remember that gold leaf is green and so affects the tint of that which should be orange—and also the blue somewhat. Again, direct rotation diminishes the light and then succeeds blue. Must examine these minute changes carefully. As to the general effect of gold leaf, that is all very clear and distinct.

15140. Proceeded to examine other things as to the general effect only—they being on glass and noted in the *inclined position.* Films by phosphorus: No. 205, corrugated—same as gold leaf but not so bright an image. No. 372, a fine regular gray film—as gold leaf but the rotation of the analyzer for extinction not so much as for gold leaf. No. 375, fine regular film. On revolving the analyzer direct, red and blue tint came on in succession rather than a marked minimum of light, and so with all the films. Is this like the action of quartz—except as it includes both rays? No. 284 is a film variable in thickness, but the effect is the same in kind in all parts. We cannot consider these films altogether as

continuous and therefore equivalent to gold leaves. Probably light passes which is unaffected by the gold and mingles with that which is affected by it.

15141. *Palladium.* The films of this metal, which are excellent and have high reflective power and are probably considerably continuous: Nos. 45 and 46 act excellently well, as gold.

15142. Took the *deflagrations of gold*, which must be considered as diffused separate particles in one plane. Nos. 319, 400—they acted exactly as the leaf and films of gold had done—even in the distant and very thin parts of the deposition. It was very striking to contrast the thinnest parts of these films with the neighbouring parts of glass cleared from it.

15143. *Deflagration of Silver.* No. 342—just as the former.

15144. It must be remembered of these metallic dusts that the particles are not like those of starch or crystals, for they have no action whilst in a plane perpendicular to the polarized ray. Nor have they a better action for being in a thicker layer—it is only the two surfaces of the layer of particles which act—this must shew a joint action of two or many neighbouring particles on one undulation of the light ray.

15145. A saturated solution of Sulphur in Sulrt. Carbon is much denser than the Sulphide of carbon alone and when they are mingled strong striæ are formed. The angle of total reflexion between the two and glass is   .

15146. Removed the Sulphide Carbon and replaced it by this medium, the *solution of Sulphur in Sul. Carbon*—it produced *no rotation* or change in the polarized ray. Thin plate of Crown Glass (as were all these plates employed) across the ray perpendicular to it: no effect. When more and more oblique, it at last depolarized a little and required a little *direct rotation* of the analyzer to put out the light, which it did very well, after which orange succeeded and then full brightness; this appears to be the same result as for the former medium. Another glass produced the same effect. Total reflexion came on before the angle was very great—perhaps with 30° to the ray.

15147. *Gold leaf on glass.* Nothing whilst perpendicular. When inclined, the light appears. The rotation of the analyzer does not seem to find out any minimum position other than that it has

without rotation, as if no displacement of the plane of polarization; but direct rotation goes on to pale blue and reverse rotation goes on to orange or warm tints: the gold leaf was inclined about 45° to the ray. At greater inclination perhaps displacement of the plane of polarization. Another gold leaf—the direct rotation goes to blue and a good way on—the reverse rotation of the analyzer to orange.

15148. It is probable that a saturated solution of phosphorus in Sulphide of carbon would give a fine medium as regards high refractive power for experiment.

15149. Removed the medium and now replaced it by *Ether* for comparitive trial of Glass and gold. A plate of glass inclined gave an image which required a very little direct rotation of the analyzer to reduce it to a minimum. A plate of glass with gold leaf on it, inclined, produced an image which required a very considerable direct rotation of the analyzer to reach the minimum, which was blue. In all these observations of colour through gold must remember the colour of the gold itself, which will have its effect. So ether acts as the other media, though perhaps not so well, as the glass is not so thoroughly neutralized by it.

15150. Removed the Ether and replaced it by *Camphine*, and observed the image with that alone. It rotated the ray and made the image produced a blue; then direct rotation of the analyzer continued brought that down to a minimum and then the red image rose up, afterwds. passing to white. If the rotation of the analyzer was reversed, these changes were reversed, and if the rotation was continued reverse beyond the normal position, the blue due to it grew up into a white. This gives a simple standard for comparison with this camphine.

15151. Adjusted the analyzer so as to give the minimum (purple) tint. Then introduced a glass plate perpendicular to the ray. Even in that position it changed the tint a little on to red; when made oblique it changed a little more to red. Direct rotation of the analyzer carried it on to stronger red, and in fact it required a little reverse rotation of the analyzer to reach the minimum of light, a little more then producing blue. So the glass alone, whether perpendicular or oblique, undid a little of that which the camphine had effected.

15152. Now gold leaf on a glass plate into the camphine: whilst perpendicular it did nothing, or if it did the gold was too opaque to let any image pass; being placed oblique, a good image appeared. Now direct rotation of the analyzer caused the image to diminish slowly and then it passed to blue, feeble; whilst reverse rotation, etc. etc. See 15158 and onwards for precise effects.

Must look at all this again, remembering what the gold and glass tints would be if the camphine tints were not there. Because of them, it is better to make these comparisons of gold and glass in Sulphide of carbon, which does not rotate and so produces no tints.

15153. Refractive powers. Vacuum 1.

√Air	1·000294	Castor oil	1·490
Water	1·336	Oil cloves	1·535
Alcohol	1·372	Oil Cassia	1·641
Ether		√Sulphide Carbon	1·768
√Oil turpentine	1·475	Flint glass	1·830
Plate glass, average	1·528	Sulphur	2·148
√Crown glass, Do.	1·530	Phosphorus	2·224
Quartz	1·548		

15154. Can consider gold leaf itself as very little like a flat plate—it is only the average effect that can be had—across a hole very irregular. As to the thickness, also a very irregular thing. Films on glass are more like flat plates, but then they are full of holes or spaces between the particles.

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15155. The ruby jelly CI (15088) has become darker I think. I melted part and made four specimens to dry; two in watches glasses, Nos. 417, 418—and two on plates, 421, 422. I also took the ruby jelly No. 2 of (15115) and dealt with it in the same way, making the specimens Nos. 419, 420, 423, 424.

15156. Tried the refractive power of Water, Alcohol, Ether, Sulphide of Carbon and sol. of Sulphur in sulphide of carbon by a cube of glass and the phenomena of total reflexion. Water was far less than Alcohol or Ether—the two latter were very close together. Sulphide of Carbon again far surpassed them, but it and a saturated solution of Sulphur in sulphide of carbon seemed very close together. The cube was of flint glass.

15157. Arranged the little lamp—the Nicol polarizer—the glass cell and the Nicol analyzer in place as before (15089). A good part of the cell selected—Sulphide of Carbon put in—a little (very little) light comes in, but the plane of polarization does change its place. No colours appear—the change of plane is very little.

15158. Adjusted the analyzer to minimum. A single *crown glass* plate into the cell—perpendicular to the ray—no effect—inclined up to total reflexion—still nothing—very clear result; so that the glass and the sulphide very nearly compensate each other.

15159. Now *gold leaf* on crown glass into the cell—produced the effect well when oblique, upon both inclinations and with both sides of the plate to the eye—alike in degree. Now take one direction of inclination about the vertical axis—it brings the light in—and then it requires analyzer rotation to the right or direct to reduce the light to a minimum, and beyond that cold tints appear and brighten up into white light. So if the analyzer be adjusted to the minimum, right handed rotation produces cold tint and left handed rotation produces warm tint. This very constant, shewing a rotation of the plane of polarization.

15160. Sometimes it seems to me that, leaving the gold plate in the cell and fluid and waiting a time, the effects are less and the requisite amount of righthanded rotation to reach the minimum less, though the tints are always on the same sides. But whether this is due to fatigue of the eye or a real effect and due perhaps to the cooling of the plate—for the cell by evaporation is below the common temperature—I am not sure.

15161. If the analyzer be adjusted to extinguish the polarized ray before the gold leaf be introduced and then the gold leaf be introduced obliquely, a very little rotation reverse brighten[s] the image produced at once, but rotation to the right or direct has to be considerable before the image is equally brightened; shewing in this way that direct rotation has to pass a minimum before it can exalt the passing ray.

15162. A thinned gold leaf, No. 68, acted just as the former (15159), except that the minimum presented very little reduction: direct rotation of the analyzer produced cold tints and reverse rotation warm tints. As to the amount of reduction of the light at the minimum position of the analyzer, that must of necessity

vary uncertainly, for as all these leaves, films, etc. have holes and pass light unaffected by the gold, that will rise up at once by motion of the analyzer and mingling with the other will prevent the true minimum of the latter from being obtained. It is probable that a very thin continuous film will give thorough extinction at the minimum, when a thicker film or a leaf because it is porous will not give extinction (15165, 6).

15163. Rings of thin copper wire had gold leaf stretched across them, being held by gum. Here there was no glass, only a film of gold in the medium–the effects were exactly the same as when the gold was on the glass.

15164. The position of the plane of polarization and that of inclination may be recorded thus. Looking along the ray towards the lamp, the position of the Nicol is represented by the section, and consequently the light is polarized in a plane coincident with *ab*. Now *cd* is the vertical axis about which the plates of glass, gold, etc. are turned to incline them to the ray. As before said, when *ab* or *ef* coincide with *cd* ( ), then inclination of the plates does nothing; but when *ab* or *ef* are inclined 45° with *cd*, the effect of inclination is a maximum. So there are four quadrants which ought to alternate in quality, a result which ought to coincide with a difference produced by the directions *ab* and *ef* changing places one with another. So commencing in the position above and putting an inclined gold leaf in the cell, it produced a warm light which by direct rotation of the analyzer was reduced to a minimum and then rose up with blue or cold tints. Turning the polarizer 90° so that direction *ef* now took the place of *ab*, the introduction of the gold leaf oblique now brought in also a warm light, which by *reverse* rotation of the analyzer was reduced to a minimum and then rose up blue or cold in tint. Now turning the polarizing prism 180°, the effect was precisely the same as the [latter]–left-handed or reverse rotation of the analyzer being required for the minimum. Rotation of the polarizer by 90° more brought back things to their first state, and now direct rotation of the analyzer was required to compensate for the effect of the gold leaf. Good.

15165. As to a very thin film or leaf being effectual if continuous (15162), No. 15 is a fine film, beautifully regular in its gradation

of thickness from one end to the other—it has a good slate gray colour and considerable golden reflexion at one end—at the other the transmitted tint and the reflexion are hardly sensible. Yet this film when introduced inclined was as far as I could judge equally effectual in every part—if any thing, more at the thinner than the thicker end—perhaps because the thicker part exerted a higher amount of ordinary obstruction to the passage of light. This layer must be infinitessimally thin, yet its two surfaces (for it has nothing like structure) seem to act separately on a light undulation.

15166. I have a beaker which has been used with the gold fluids—its lower part has a film of gold on it and is stained ruby ( ); the film is exceedingly thin and exhibits very faint traces either of colour or reflexion, but it seems very continuous. This was broken up and a piece selected, No. 425. When introduced inclined, it brought in the image of a good bright red—direct rotation of the analyzer produced *complete extinction* at the minimum place—and then raised up good blue—the reverse rotation of the analyzer returned these effects. There is good blue on the right of the minimum and good red on the left. Here see the effect of a continuous film (15162).

15167. Gold leaf on glass—put a second glass over it and went round the edges with thick gum to fasten them and exclude the medium. As the glass is like the Sulphide Carbon, this arrangement ought to present the effect of a plate of air in one part and of a compound plate of air and gold in the other. When perpendicular to the ray, nothing happened at either part. When inclined, the light was depolarized well at the air part; as the inclination increased the depolarization increased; at about angle of 45° with the ray the light became fine ruby and then disappeared, total reflexion coming on. When the gold was up, less light passed but just the same effects occurred. It was as if the gold merely obscured the light. Must look at all this more closely, to see if there is rotation and if the gold adds to or takes from the air action. Whether the air plate was inclined on one or the other side of the ray made no difference in the result (15296).

15168. Prepared two cells of two plates of glass, each separated by a card thickness—the edges were attached first by soft cement and then this was coated externally with thick gum to protect

it from the sulphide Carbon. Introduced portions of LXXXIII C and LXXXIII D deposits (15135) into them; obtained two very good plates of good strong colour and placed them in the medium—but whether perpendicular to the ray or oblique to it, they produced no trace of action on the light. So whilst diffused, these films can do nothing; they must be aggregated into a film—probably because an equivalent to lateral continuity is required. Made the cells 426 and 427.

15169. Have put portions of these same fluids on to two glass plates and allowed them to dry; LXXXIII C is marked 428 and LXXXIII D made 429; both portions are very badly spread. Nevertheless, both acted on the ray when oblique in the sulphide—and by shifting them so that the ray should fall on the parts of the glass either clear or holding the film, the gold effect was strikingly made out. No. 428 depolarized, bringing in a red image as before; by moving the analyzer either right or left, the light rose up so rapidly that I did not observe the colder tints. But with No. 429, the cold tints produced by direct revolution and the warm tints by reverse revolution were clear enough. So though the particles whilst diffused could not act, they do act when dry and in a layer—both shew a dull golden reflexion, shewing their power as layers or film, i.e. their lateral connexion in their action on light.

15170. Agate pressure gave to both these films an increased reflexion and a green transmission.

15171. Raised *mercury* in vapour and caught it upon glass plates. Succeeded in getting specimens in which the heat had not given depolarizing power to the glass. But these plates had no power of affecting the light. Has the liquid state of the mercury and orbicular form of the particles any thing to do with this? As a film, the coat was evident enough, but the particles were too fine to be distinguished by a glass, in even the thickest part—some parts were very thin.

15172. *Arsenic*—a piece set burning without flame on wood—and then glass plates held close over it. Obtained deposits of the metal—translucent, etc., but they gave me no signs of any action on light.

15173. There are various bronze powders, Red, orange, yellow,

white and green—being powders of copper, brass, tin, etc. Endeavoured to lay them on glass plates with and without water. I obtained no action on light from any of them.

15174. *Carbon* smoke of a candle did nothing. Zinc smoke did nothing. Antimony smoke did nothing. Per oxide of iron, i.e. rouge, did nothing. These seemed to lay very flat and close on the glass plates.

15175. A piece of red glass Moigno sent me did nothing.

15176. That a film of gold should transmit, after polarizing it, a ray the *whole* of which is polarized, which glass never does, is no doubt connected with the fact that metals reflect so powerfully, and in that respect the facts of glass and gold are the converse of each other.

15177. Will not a reason appear why reflexion by gold causes circular polarization? Is there any relation to the assumed fact that metals reflect the same amount of light at all angles?

15178. Sulphide of carbon compared as to refractive power against a strong solution of phosphorus in sulphide of carbon by the use of the flint glass cube (   ); the solution was highest, but both come too near the flint glass to be well distinguishable.

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15179. Constructed a small glass cell about $\frac{1}{6}$ of an inch in thickness, with sides of very thin Microscopic glass; placed it in the magnetic field of the great magnet, excited by 20 pr. Grove's plates, between my pointed poles. Adjusted an Electric lamp excited by 30 pr. of plates so as to send its ray along the top of the poles and along the magnetic axis and through the cell. Placed a Nicol prism as polarizer and another as analyzer. Selected a fine gold deposit, LXXXIII D (15086, 135), blue, and filled the cell with it, so that the polarized ray traversed the gold fluid—the latter was as dense as I could make it to allow the intense ray to pass—then excited the magnet and looked for any effect of rotation of the ray or other effect. The effect was very small, almost doubtful, but it was this: when the light in the analyzer was out, putting on Magnetic force brought it in a trace and revolution of the analyzer direct put that out again. Dr Tyndall

helped me and looked for the results; we both saw it alike, though both were doubtful.

15180. List of objects, continued from 3042[1]; on to p. 3173[2].

433, 434. Arsenic, triturated. 15184, 220.
435. Bismuth, triturated. 15184, 220.
436, 437. Antimony triturated. 15184, 220.
438, 439. Purple of cassius, XLVIII. 15184, 221, 9.
440, 441. Blue deposit by salt, LXXI. 15184, 222, 30, 9[3].
442. Palladium films, many. 15185, 215, 9.
443 to 447. Fine gold leaf. 15183, 4, 214, 36, 59, 72.
448. Silver films, many. 15188, 216, 9, 65.
449. Platinum films, many. 15189, 217.
450. Platinum film rubbed up into powder. 15189.
451. Rhodium film. 15190, 3, 218.
452. Sulrt. Palladium. 15198.
453. Sulrt. Silver. 15199, 228 [*a*].
454. Sulrt. lead. 15200, 26.
455. Sulrt. antimony. 15201, 23.
456. Sulrt. copper. 15202, 24, 64, 73, 4.
457. Sulrt. Mercury? 15203, 25.
458. Sulrt. Platinum? 15204.
459. Sulrt. Gold? 15205, 27.
460, 461. Deposit of LXXXIII C, dried on glass. 15231, 68.
462. Film of Gold by hydrogen. 15241.
463–466. Films of palladium by hydrogen. 15242, 67.
467–470. Films of rhodium by hydrogen. 15243, 66.
471, 472. Films of silver by hydrogen. 15244.
473–477. Platinum films by hydrogen. 15245, 69.
478–480. Gold films by hydrogen. 15254.
481–483. Palladium films Do. 15255.
484–485. Silver films by hy. 15256.
486–488. Iridium films by hy. 15257, 65, 70.
489–494. Jelly, ruby, etc. with gold. 15278.
495, 496. Gold leaf heated in oil tubes. 15284, 307.
497–505. Gold leaf on plates heated at a gas lamp. 15292, 308, 38.
506–508. Do. . . . . heated in an oil bath. 15295, 309, 10, 1, 38, 51.
509–511. Two and three golds on glass, heated, lamp. 15312.
512–514. Fine gold leaf on Mica, heated. 15314.
515. Fine gold leaf on Rock crystal, heated. 15315.
516–517. Fine gold leaf on platinum and silver, heated. 15316, 7.
518–523. Copper defln. in hydrogen. 15324, 5.
524–529. Palladium Do. . . . 15326.
530–532. Platinum Do. . . . 15327.
533–538. Iron Do. . . . 15328.

[1] I.e. p. 99. [2] I.e. p. 208.
[3] Reference probably belongs to next line.

539–544. Lead Do. . . . 15329.
545–550. Tin Do. . . . 15330.
551–554. Aluminium Do. . . . 15331.
555–560. Zinc Do. . . . 15332.
No. 504. Heated gold leaf in chlorine gas. 15345.
Nos. 561–5. Copper films by oil. 15346.
566, 567. Do. . . . coarse. 15346.
568–570. Double copper films. 15346.
571, 572. Dry gold jelly, blue on surface, No. 1. 15114, 349.
573, 574. Dry gold jelly, violet, No. 2. 15114, 349.
575, 576, 577. Dry gold jelly, salt, No. 3. 15114, 349.
578, 579, 580. Do. . . . salt, brown gold down, No. 4. 15114, 349.
581, 582, 583. Do. . . . blue, No. 5. 15114, 349.
584, 585, 586. Do. . . . fine ruby, No. 6. 15114, 349.
587, 588, 589. Do. . . . blue, salt, No. 7. 15114, 349.
590, 591, 592. Do. . . . fine ruby, No. 8. 15114, 349.
593, 594, 595. Do. . . . very poor, salt. No. 9. 15114, 349.
596, 597, 598. Do. . . . very poor, No. 10. 15114, 349.
599. Gold leaf heated in pure oil. 15352.
600, 601, 602. Gold leaf on rock crystal, heated at lamp. 15353.
603–608. Gold leaf on glass in Muffle. 15356.
609. Silver leaf on glass in Muffle. 15357.
610. Rock crystal and gold sol., heated. 15358.
611. Do. . . . and gold leaf, much heated in Muffle. 15359.
612, 613, 614. Do. not so much heated. 15368, 70, 2.
615. Silver leaf on R. crystal, heated in Muffle. 15368.
616, 617. Silver leaf heated in Muffle. 15370.
618. Do. . . heated on surface to fusion. 15370.
619. Do. . . in alcohol for powder. 15370.
620. Tube and silver leaf, 6 leaves. 11373.
620 *a*. Do. . . . . 8 leaves. 15374.
621, 622, 623, 624. Fine Golds on R. Crystal, in muffle. 15375.
625, 626, 627, 628. Silvers on R. Crystal, in muffle. 15375.
629–635. Gold wire deflagrated in hydrogen. 15403.
636–641. Ruby Jelly made for F. E. June 1857.

15181. Replaced this deposit by another ruby, LXXXIII C (15086, 135) and made this a little thinner that more light might pass. Found the same effect rather better and in the sáme order. Then replaced the gold solution by water to obtain the effect of the cell and fluid. The same results were obtained, but more easily because of the quantity of transmitted light. Finally put a piece of heavy glass there. Like effects of good strength were obtnd., but exactly in the same order. I believe the gold deposit went for nothing—and that the effect seen was due to the water and cell.

15182. But in considering the expectation of any rotation, we must not forget how small a quantity of gold was there and that the magnerotation effect depends upon the bulk of the body.

15183. Have some pure gold leaf from Mr. Smirke, beaten by Marshall—he gives it as averaging 17 dwt. or 408 grains per 2000 leaves; i.e. 0·2 of a grain per leaf. It is a fine rich orange yellow—very soft and flexible, so that it requires much more care in handling it on the cushion and hangs on the knife with different draping folds to ordinary gold leaf. It is sin[g]ularly mottled in the middle of the leaf, some parts being seemingly 3 or 4 times as thick as other[s]. At the side it streaks out like other leaf. Colour a fine warm green by transmission. Laid on cyanide of potassium; it dissolved away, leaving no trace. Laid on chlorine solution; it also dissolved away, breaking up as it became very thin and leaving no trace of chloride of silver.

15184. Have been preparing many glasses for experiments with polarized light—in Sulphide of Carbon. The metals first mentioned in the following list were rubbed up very finely in a mortar with a little water, the lighter particles washed off and when they had settled, they were put on the glass with a little water and left to dry.

Nos. 433, 434. Two Arsenics—much white oxide seems to have formed (15220).

No. 435. Bismuth (15220).

Nos. 436, 437. Antimony (15220).

Nos. 438, 439. Purple of Cassius, XLVIII (15221).

Nos. 440, 441. LXXI, deposit blue by salt.

Nos. 443, 444, 445, 446, 447. Pure gold leaf.

15185. Put a weak solution of chloride of Palladium into a dish and floating particles of phosphorus on to it. After intervals of 3 hours, 6 hours, 12 hours, etc., took off specimens, which are *all* numbered 442. Many concentric portions were formed and here I had the circular rings of colours, the effect of *thin plates*. Very beautiful whilst floating on the fluid and also very well after being transferred to the glass plates. The transmitted colour of Palladium is Indian ink, from faintish shade up to blackness. The thinnest film has by reflexion the polish of the glass and a good

metallic reflexion; as they became thicker they became less brilliant, but long before they refuse to transmit light appear perfectly black and dead by reflexion.

15186. Now taking a part of medium thickness and using Agate pressure on it—that part became as highly reflective as any other part—very brilliant; and at the same time less light was transmitted but the tint transmitted was the same as before—not coloured. So seems that transmission and reflection here are not connected with *production* of colour—but whether or no, the principles of action are most likely the same here as in gold. If so, then pressed gold shews the true characters of the metal and the other forms of gold—the effect of the joint action of the inside and outside of particles at the same instant upon an ether undulation. Think this is most probable.

15187. The palladium film when thin is pretty stout upon the fluid and breaks brittle, i.e. with sharp shining long cracks.

15188. Sol. Nitrate of *silver*—treated in the same way—films easily formed—concentric rings effect of thin plates soon appear. The transmitted colour is from warm brown or sepia in the thinnest films to blackness in the thicker, but the thick films soon pass into heaps of mossy silver close to the phosphorus—like a sponge, so loose. The reflexion is scarcely metallic in any case—the thinner parts are polished as the glass is, the thicker a dead black. Agate pressure brings out the polish very well, and then in favourable places the light transmitted is seen to be less and of a dark blue or slate blue colour. No. 448.

15189. Sol. Chloride of Platinum—treated in the same way—After much longer time, signs of a film and of concentric ring colours round the phosphorus. On examination found a very thin film all over, and took off specimens and numbered them 449. They are scarcely visible on the glass except near the center of the ring part, and there very little colour or lustre even when on the fluid and before transfer—hardly sensible. The film, when on glass nearly clear in colour, has a rosy tint perceptible on looking through it at white paper. When the solution was poured out of the basin, a thin film was caught on the bottom, and it shewed the same rosy or purplish tint. This being rubbed up by the finger aggregated into a sediment of which part was put

on a glass and made No. 450, but it was poorly spread, for the particles had welded together. The rest of it examd. by acids proved to be metallic platinum.

15190. Solution of Chloride Rhodium dealt with in the same manner. In 3 or 4 hours, had given a beautiful film of metal in concentric rings, varying in reflecting and transmitting power and also in colour. Those which reflected, transmitted little light—those which transmitted, reflected little light—one might have thought that there was no metal in some of the rings between other rings that reflected brilliantly; but I believe the metal was there, only in such thickness as transmitted the light. These rings deserve close attention. Put on more. No. 451.

15191. The application and effect of the S. of Carbon in these depolarizing experiments compares and measures in some degree the high refraction force of the metals and so distinguishes them. Even if other bodies, as glass and metallic oxides or sulphurets, have like powers, as probably they ought to and will have, yet there may be a distinction by the amount of inclination and the medium required.

15192. In respect of the position when a plate of glass or gold depolarizes (15164), it is a little difficult to me to recognise the physical cause of non action when the plane of inclination is transverse to the plane of polarization—and why it does not then produce most effect. Instead of which, that is an indifferent position separating two quadrants of opposite rotation (15164).

15193. Made Rhodium solution, feeble strength but coloured red brown—floated particles of phosphorus (15190); in an hour was going on well. In about 6 hours took off three concentric films (dropping the phosphorus to the bottom of the solution) and left one center to go on all night. The three, made also 451, were excellent films. Next morng. the left one was excellent—discs like and shewing thin films action.

15194. The production of plates of metals, etc. very greatly influenced by the *strength* of the solution and the *time* of action.

15195. Filtered the Platinum solution (15189) to remove fragments of films, etc. and left particles of phosphorus on the surface. Platinum films next morng. were there, very thin. Knocked the phosphorus through but it clung very hard. Films almost

invisible—thin plate action. Made all the platinums No. 449. Is the film phosphorus only? (   ).

15196. On examining the various concentric plates of gold on glass, I find traces of effects of thin plates, but they are feeble, probably because hidden by the colour of the film and because the softness of the gold easily admits of disturbance of the particles in manipulating with them.

15197. Proceeded to prepare some films of metallic sulphurets, or rather to reduce some solutions of metals by an atmosphere containing sulphuretted hydrogen. Put the weak solutions of the salts into dishes or glasses, and enclosed them all under one large bell glass and then threw in a little sulphuretted hydrogen: films soon formed on some, more slowly on others. The following are first results and were numbered as marked.

15198. No. 452. Sol. chloride of *Palladium*. Film excellent, and in appearance and character exactly like those of Palladium by phosphorus. I doubt whether there is any sulphur in it but we shall see.

15199. No. 453. Sol. Nit. *Silver*. Film very feeble and poor—easily broke up in the washing—colour marked and brown by transmitted light. Solution is probably too feeble.

15200. No. 454. Sol. Nit. lead—a good grey film, colour of galena, having metallic lustre but very brittle and easily breaking up—is evidently a sulphuret.

15201. No. 455. Sol. Emetic tartar—Antimony. Some precipitate but also a film of very slightly aggregated particles, apparently crystalline—of an orange colour and good lustre—a sulphuret.

15202. No. 456. Sol. Acetate copper—a good film—of a pale brown colour—looks adherent—is thin.

15203. No. 457. Sol. Cor. Sublimate. Very thin film—very little colour—adheres together well—may it be sulphur?

15204. No. 458. Sol. Chlo. Platinum—doubtful if any film—the solution is too weak.

15205. No. 459. Sol. Chlo. Gold—very feeble if any film—solution is too weak.

15206. Looked at the ten Jellies (15115).

1. Body not coloured—top surface brilliant golden reflexion—by reflection—perceive particles within the body.

2. Body fine ruby—I think it becomes continually deeper. No reflexion of gold on the surface.

3. Body not coloured—upper surface brilliant golden reflexion.

4. Body has a little ruby tint, but is brown and muddy throughout, as if gold down in large particles.

5. Body slight pale blue—surface not metallic.

6. Body fine ruby, not deep—surface not metallic.

7. Ruby fine but pale—surface not metallic.

8. Body beautiful ruby—surface not metallic.

9. Body blue and turbid like prec. by acid sol. of iron. Surface has no metallic lustre.

10. Body dirty deep brown throughout—somewhat reddish—surface not affected.

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15207. Proceeded to arrange the polarized ray, etc. ( ) with the cell and the medium Sul. Carbon for further experiments as before. Examined the fluid when put in as to any rotation possessed by itself (15157). It possesses no rotating power nor any depolarizing effect—a little trace of light appears due to malformation of the glass cell, annealing, etc. and perhaps in part to the change of temperature by evaporation.

15208. Polarized the ray in an inclined plane \ as before (15164), revolving the plates experimented on round a vertical axis.

15209. Tried mica in the air for a supporting plate for films, but could find no inclined position of no depolarization sufficient for my purpose. Its own action on light too good.

15210. When either gold film or clear glass is inclined accurately in the plane of polarization or at right angles to it—there is no depolarizing effect produced at any angle of inclination.

15211. Compared air, glass, Sulphide of Carbon and gold together as to the direction in which they rotate the ray when they depolarize it. Air between glass and sulphide of Carbon in the cell No. 426. Air is the plate here whilst glass or sulphide of carbon is the medium. There was good depolarization—which required the analyzer to rotate direct to compensate for it; there

was a fair minimum place on the left, but not much signs of colour produced. Using the same cell No. 426 where the gold was, is equivalent to employing a compound plate of air and gold in the medium. The depolarization occurred—and still direct rotation of the analyzer was required, but much more than before and there was some colour produced. Then employed a plate of glass in the air as medium—there was depolarization, still requiring direct rotation of the analyzer to compensate for it. Gold leaf in air does the same thing, so that all these contrasts, strong as they are, give the same results as to direction. They should do so, for an absolute reversion, if it occurs, would still bring a quadrant of the same character on to the same position of the plate.

15212. Used a glass plate in air and revolved the polarized ray so as to obtain experimentally proof of the alternate quadrants.

15213. The effect of a plate of glass when perpendicular to the polarized ray is nothing if all be at the same temperature (15137).

15214. Fine Gold leaf, No. 445, 447, acted as ordinary gold leaf in the medium. Much direct rotation of the analyzer was required before the minimum was obtained—which however was not very dark—then blue came on by further rotation.

15215. *Palladium films*, No. 442. It depolarized a little—a little direct rotation of the analyzer first extinguished the image and then brought in cold tint, blue. The thin part of the film did as well as a thicker part—with a much thinner plate of Palladium a very good effect, better than a thicker part. Employed a circular concentric film. A certain thin part did best, but not the thinnest—the thinnest are clearly not the best—some are so thin as to be almost insensible by reflexion or transmission of light. Nor is the thicker or the dull part good. Any part having a dull or dead surface falls in power. The film is best when it has bright metallic surfaces, and no doubt also when it is continuous and not permeable. Many results in other cases shew how a film which adheres together as one thing surpasses in effect a rotten fragile porous film. All this shew[s] the action of an uniform plate with good surfaces and without specific action of the particles.

15216. *Silver films* (15188). Three specimens No. 488 (   ) were employed. They acted as Palladium, etc. Direct rotation

of the analyzer was required and cold tints followed the neutral or minimum place. Here a good transparent pale brown film, thin but continuous and with good metallic face, was better than thicker parts. Rough or dead surfaces were bad.

15217. Platinum films (15189). Numbered 449. These films are difficult of formation—an exceedingly thin film forms and then the process goes on very slowly. The existence of the film is shewn by the manner in which the surface of the solution moves when the films are taken up on the glass plates; for a little space round the phosphorus the rings are seen and where thickest the platinum film will shew a yellow colour by transmission. When on the glass, the central or thicker part may be seen by the eye, but at the more distant parts where I believe film to be, there are no sensible signs of it to the naked eye. One plate, $\frac{449}{1}$, produced a depolarizing effect at the middle thicker parts of the same kind as gold, etc., but not in other parts where I believed a thinner film to be. No. $\frac{449}{2}$ was a very thin film visible by a tint when over white paper and also before removal from the bath. But its effect on the polarized ray very doubtful—perhaps a little change of tint. Another plate, $\frac{449}{3}$, with a distinct yellow spot of film in the center, gave a doubtful effect even at the spot—though perhaps it came in at the highest inclination but that place was a little rough by corrugation of the spot. No. $\frac{449}{7}$ had a good central part about half an inch across and yellow by transmitted light. The effect was distinct but small. The plate even there is probably too thin. The specimen 450 (15189) of platinum particles did nothing.

15218. Rhodium films (15190, 3). Nos. 451. Both $\frac{451}{1}$ and $\frac{451}{2}$ gave good results of the former kind—same direction, rotation, etc.

15219. Agate pressure acts on some of the other metals as it does on gold: thus it gives good reflexion to dead surfaces of Palladium and silver, just as it does to gold, and then it diminishes the transmitted light often and in silver tends to give a dark blue colour ( ). It has the effect also of improving the films in their action on a polarized ray; both No. 442 palladium and 448 silver shewed this effect. This accords with the idea that it is a plate with flat reflecting surfaces that is wanted, both for ordinary transparency and colour and also for these depolarizing effects,

which depend on the regularly formed mass and not on the individual particles.

15220. The specimens of pulverized Arsenic, bismuth and antimony (15184) Nos. 433, 434, 435, 436, 437, did nothing, not having that form of a plate which is requisite.

15221. The Purple of Cassius (15184), Nos. 438, 439, fine dry films, have a deep purple brownish golden reflexion–but were too thick, obscuring much light. One of the plate[s] produced an effect (the effect), bringing in a red image (because of the colour of the Cassius), but proved to be the effect because direct rotation of the analyzer carried the image on to cool tints–and reversing the rotation brought back the tints to red again.

15222. The deposit LXXI (15184) Nos. 440, 441. The specimens have dried, presenting a dead golden reflexion–but the layers are not flat. Both gave very imperfect and confused effect, a little depolarization but confused, and are not continuous plates or the equivalent of such.

15223. Experimented with the films of Sulphurets prepared Yesterday (15197–205) from metallic solutions and sulphuretted hydrogen gas. The *Sulphuret of Antimony* (15201), No. 455, an orange coloured film–did not affect the ray when perpendicular to it, but when oblique acted as the other bodies, Gold, glass, etc.

15224. *Sulphuret of copper* (15202), No. $\frac{456}{1}$ and $\frac{456}{2}$. This sulphuret forms a good adhering film which, when on the glass, has excellent surfaces, though very thin–it transmits a pale brown colour. It depolarized effectually, bringing in a blue or cold image; then right handed rotation of the analyzer brought in a warm tint. With one of the plates the effect was very good and as follows: the image being out, the use of the oblique film brought in the light–then a very little direct rotation made that image a dark blue, a minimum, and then a red and then brightness–reverse rotation of the analyzer passed back through the series. Is this the proper order of all the cases? (15214).

15225. *Sulphuret of Mercury* (15203), No. $\frac{457}{1}$ and $\frac{457}{2}$. The films are brown by transmitted light and look bright and continuous, reflecting a good image. Produces a very good effect–requires direct rotation to compensate the effect.

15226. *Sulphuret of lead* (15200), Nos. $\frac{454}{1}$, $\frac{454}{2}$ and $\frac{454}{3}$. The films

are metallic looking but do not adhere together and the medium floats off much of it from the glasses. It broke up even in the gathering, 15200. The effect on the ray was very doubtful—I believe only because of the badness of the films.

15227. *Sulphuret of Gold* (15205), Nos. 459. There are here two small spots of film evident *a a*. They look like accidents from some particle of dust rather than a film formed by the Sul. hydrogen. They produce the effect—but the other parts of the glass or film, if there be one, produces nothing—doubt the existence of a film.

15228. The Jellies (   ). The simple film of dried jelly on glass, Nos. 429, 428, 432, do nothing when they are perpendicular to the ray; when oblique they produce exactly the effect of glass in air—they do not displace the plane of polarization, i.e. they do not sensibly rotate the ray but they bring in the light and perhaps colour it. Those jellys which are rendered ruby or blue by gold, as Nos. 423 and 421, have exactly the same effect in the smooth parts, but the light is coloured by the gold tint. The jelly acts as any other plate would act, but the gold in it (diffused) does nothing as regards the polarity of the incident ray.

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15228 [*a*]. Sulphuret of Silver tried on Saturday, No. 453 (15199). It was exceedingly thin and there was no sensible effect.

15229. Agate pressure applied on to the Purple of Cassius, Nos. 438, 439—on the thinnest parts of the films giving an increased reflective power and a green transmission, but the particles are large and coarse and the effects poor.

15230. Agate pressure on deposits LXXI, Nos. 440, 441. More reflexion, but the particles are too large for transmission and always appear black and coarse in that direction.

15231. Put LXXXIII C on to plates to dry, Nos.   . These by agate pressure greened well in parts that adhered to the glass but most of the gold rolled off on to the agate. Unheated and heated gold both green alike by pressure.

15232. Have glasses jellied on one side, Nos. 428, 429, 432; also prepared two others jellied on both sides, Nos.   (15228). Then put them into the polarized ray in air, inclining them at angle of 45° to the plane as before. They were the same as glass

alone and required direct rotation of the analyzer to compensate effect. Verified the succession of quadrants of opposite rotations with these glasses.

15233. Proceeded to work with gold leaf, etc. etc. as to their power of *producing* a polarized ray and its direction–and worked first in air. Here there was no polarizer–only a screen with a small hole to let a lamp ray through–then the plate–then the analyzer. The pure gold leaf was stretched on a ring of copper wire (15163) and rotated round a vertical axis. When it was perpendicular to the ray there was nothing, when oblique there was polarization of the ray; when very oblique the passing ray is almost wholly polarized and the plane of its polarization is vertical, i.e. it is perpendicular to the direction of the inclination as in the case of glass, water, etc. The analyzer stands thus when it lets the ray pass, the inclination of the gold leaf being about the axis *a b*. No colours appeared here.

15234. Plate of crown glass in air produced the same effect; but so much unpolarized light passes as to obscure the effect and but little apparent difference occurs at the maximum and minimum positions of the analyzer–they were the same as for the gold leaf.

15235. As I could not work with films in air because they had to be sustained on glass, proceeded to us[e] the *Sulphide of carbon* as a medium. Then the thin or thick glass plates in it gave no sensible traces of polarized light by inclination. Just before total reflexion, the transmitted light became a fine red, but that was prismatic and remained red in all positions of the analyzer.

15236. *Pure gold on glass*, No. 445 ( ) in S. of Carbon–inclined–produced polarization in a vertical plane. Could not now extinguish the light (as in air) so as to make the whole transmitted beam polar. When the glass face was toward the incident ray the effect seemed best. The difference of Gold and Sulrt. Carbon is sensibly not so great as gold and air.

15237. *Thinned gold leaf*, No. 73, did the same. Much common light passed, i.e. the image was always luminous, but changing in tint.

15238. The *Gold films*, Nos. 8 and 14. Acted in the same manner but the effect was small. No. 260 did very well as regarded change of colour but plenty of light always passes. No. 312 is a circular

film differing in thickness—the thickest part acted very well as to colour. Of course much light passes through these as they are films not continuous.

15239. *Palladium films.* No. 44 as gold leaf. No. 442 as gold leaf—the thicker part of this film obstructed more light than the thinner—but did not polarize more distinctly. Another Palladium No. 442, irregular in parts as to thickness. Nothing particular in the different parts.

15240. *Deflagrated gold*, No. 322. Acted rather well (15258).

So all these bodies polarize by transmission (15258).

15241. Put some solutions on the air pump into an atmosphere of hydrogen—to ascertain if films could be formed thus. Six preparations were in small capsules, five were in glasses (test). The following are their numbers and natures:

1. Chloride of Gold—5 drops of de la Rue's solution and water.
2. . . . . . Palladium—from the large bottle of diluted solution.
3. . . . . . Rhodium, 4 drops from shelf bottle and water.
4. Nit. silver—9 drops from shelf bottle and water.
5. Chloride Platinum—8 drops from Stock bottle and water.
6. Chloride Iridium—20 drops from shelf bottle and water.
7. Solution of Acetate of Copper.
8. „ „ Arsenious acid in water.
9. „ „ Emetic tartar.
10. „ „ Proto chloride of tin, acid.
11. „ „ Bichromate of potassa (15253).

Very soon 1, Gold, shewed trace of action round the edge of the solution in the capsule and perhaps a fine film over the whole surface, but in several hours it had not increased—No. 462.

15242. 2, Palladium—instant action—a black powder sinking—in the course of 6 hours much deposit—but also a film on the surface. On taking this off by glass, it was found so thin, mobile and without tenacity that I could scarcely keep specimens. See Nos. 463, 464, 465, 466—there was much deposit.

15243. 3, Rhodium. Action at once—dark film. After 6 hours, taken off. Made 467, 468, 469, 470—films appear in striæ as if

formed of floating separate particles coming together—very little lateral adhesion.

15244. 4, Silver. Action at once—darkish film—but it did not thicken and in six hours was very poor. See Nos. 471, 472. Could not wash it, so rotten and fragile.

15245. 5, Platinum. Quick action. Minute spots appeared here and there on the surface—then these grew and enlarged—became rough and dead in the middle but with bright shading narrow edge parts—these enlarged until they joined together and form a common crust or film—dark or black by transmitted light—dull metallic lustre on the air side but black on the glass side, except at the thinnest edges, and there it was brilliant and metallic. The whole action seems, after the first formation of spots, as if dependant on air voltaic circles. There was no common film over the whole surface and in the specimen a little breath of air is enough to detach the films of platinum from the glass. Made them Nos. 473–477.

15246. 6, Iridium—no sensible action. Nor with Nos. 7, 8, 9, 10 and 11.

15242*a*. At 7 o'clk. P.M. filtered 1, 2 and 4 and put them with 6, 7, 8, 9, 10 and 11 over the air pump with hydrogen and left them for the 27th.

15243*a*. However minute the vibration of an ether molecule, it must have a sensible relation to the ether wave and therefore a distinct relation to the very minute molecules of gold, and may well be reached by them in their action.

15244*a*. Think that green is probably the true transmitted colour of continuous gold—then the reflexion and transmission are most direct, and the rays affected most parallel and least dispersed. When lateral separation appears, as in the films, deflagrations, fluids, etc., then the rays are otherwise affected according to size of the particles, and all the colours within a certain range come in with the transmitted light. In that case must consider true transmitted colour as an action of mass.

15245*a*. If green be taken as the true transmitted colour of continuous gold, and if breaking up the continuity causes change by the consequent joint action of a center and the parts *about* it on an ether molecule—it may well be that further change of the same

kind should be able to cause still further change of the final effect on the vibration, and therefore a further change of colour from the grey of the films to the ruby of the finest particles.

15246 *a*. As to heating or pressure conferring a constrained condition on the particles of gold, I cannot find signs of that condition. A plate of it acts as a plate of glass or of air. Pure gold is very soft and may not be able to keep a constrained condition. A leaf of pure gold is very irregular in its central and other parts, shewing there heaps, and these central heaps must cause as much lateral compression as vertical compression; yet they are as green as the hardest gold leaf and as green in the thin as the thick parts, and keep green as long on the application of heat. Moreover the heat does not dismiss the green tint until it lets through much light, shewing that the effect is one of retraction and not annealing.

15247. The pure gold leaf or film is so soft that the finger or a card greens it down by a streak. Can such a draught of card be thought to give much compression vertically or more than laterally? Perhaps it may, but seems insufficient to account for the other results.

15248. Gold leaf is a very irregular thing to be considered as a *thin flat plate*, but it has the advantage of *continuity*, and the thinnest parts still act and let the most light through; furthermore, the lateral distance between a thick and a thin part is probably many times more than the thickness of the thickest, so that the inclinations of the surfaces are probably not very sudden or steep.

15249. The greening effect of pressure may probably be taken as a test of the metallic condition of the gold (and the like effect with other metals as a like test), in which case it will prove that the ruby films either by phosphorus vapour or deflagration are metallic gold—as well as the films obtained with deposits, LXXXIII, etc.

15250. If green gold is the state of gold as a really continuous body (15245 [*a*]) then the other states are divided more or less—the polish produced by pressure on dead films, etc. accords with this. But plates of the divided states, as films, deposits, etc. can polarize and depolarize. This would seem to shew that the latter actions are consistent with larger actions than the former, i.e. that the distance of effectual action is less in the latter.

15251. The mottled condition of beaten gold is hardly consistent with a great predominance of compression in one direction. When heated it looses greenness and opacity together, and lets light through, shewing change of form of the parts seen through.

15252. If continuity be a condition of the true colour of gold, it most likely has an influence in many coloured bodies.

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15253. This morning there was a little absorption of hydrogen or else perhaps a little lowering of temperature. Took down the experiment (15241). There had been no action on Nos. 7, 8, 9, 10, 11. The others were acted upon.

15254. 1. *Gold.* Very thin film—too thin to wash—the film had a metallic reflexion and transmitted a dark slate blue colour: made three specimens, Nos. 478, 479, 480.

15255. 2. Palladium—as before (15242): dark deposit and bright black film—very thin. Made three specimens, 481, 482, 483. The deposited particles clung together on moving the fluid, pouring it off and adding water.

15256. 4. Silver. Film as thin as before (15244)—no tenacity—could not wash them—not worth having. Two specimens, Nos. 484, 485.

15257. 6. Iridium. Films had formed something in the manner of platinum, from centers. Dark coloured and dead. Have three specimens, 486, 487, 488.

15258. Continued the experiments from 15240, i.e. used plates of the substances mentioned in a S. of Carbon bath to polarize a beam of common light and examined the ray by an analyzer. Resumed the *deflagrated gold*, No. 322—it acted but produced a very feeble effect.

15259. *Fine gold leaf*, No. 445. Acts—light always passes and therefore far more than in air, but the effect is well seen by the change of colours—the minimum light in the analyzer is obtained  when its position is thus, and that minimum light is pale bluish. If the gold plate be replaced by a Nicol's prism as polarizer, the  latter must be placed thus to produce like effect, i.e. at right angles to the analyzer.

15260. A bundle of thin plates of glass were then employed as the polarizer in place of the gold leaf: they had to be placed with the same inclination to produce the same effect at the analyzer.

15261. *Deflagrated gold*, No. 386: shewed the like effect produced by the change of colours in the Analyzer—the effect was but small. The same result occurred with No. 394.

15262. *Deflagration of silver*, No. 345—produced a trace of action. No. 348 gave a somewhat better result.

15263. *Heated gold deflagration*—ruby tint, Nos. 387 and 401: gave feeble traces of action, but the state of the glass interferes.

15264. *Sulphuret of copper*, $\frac{456}{1}$ and $\frac{456}{2}$: very feeble effect if any—am in doubts about it.

15265. *Silver films*. No. 47, fair effect is a thick film. No. 448, only a slight effect if any. Another 488[1]—an effect.

15266. *Rhodium film*, Nos. 467 and 469—very feeble.

15267. *Palladium film*, No. 466—very feeble.

15268. The deposit LXXXIII C, No. 460—does produce an effect but it is very small.

15269. *Platinum film*, Nos. 473 and 476—dark—no effect.

15270. *Iridium film*, No. 487—dark, not sensible to me.

15271. Now polarized a ray by the Nicol's prism; then put No. 180, which is made green by pressure, into it, but could see no difference in the green and purple part. The glass having been heated is variably active.

15272. Compared direction of rotation and consequent colours produced by fine gold (15214) and sulphuret of copper (15224); see 15211 also. Placed the Nicol polarizer thus and employed the medium Sulphide of Carbon. No. 445 fine gold was introduced. When inclined it depolarizes—then requires direct rotation of the analyzer. When the minimum is obtained then reverse rotation brings in warm tints and direct rotation cool tints. The gold leaf was revolved 180°: still the same thing occurred—it first required direct rotation, and then left rotation brought in warm tints and right rotation brought in blue tints.

15273. Now the Sulphuret of copper film No. $\frac{456}{2}$ (15202) produces a depolarizing action and requires *reverse*? rotation of the analyzer to produce the minimum: but colour[s] appear strong and

[1] ? 448; 488 is iridium according to par. 15180.

close to the minimum; the red is on the right of the minimum and the blue is on the left. The contrast between it and gold seems very clear (15298).

15274. The same sulphuret of copper on glass in air, $\frac{456}{2}$—produced an effect—and required direct rotation to compensate it and obtain minimum. Then the red appeared on the right and the blue on the left, as in Sulphuret of Carbon, but the effect of colour not nearly so sharp as before in the sulphide of carbon (15302).

15275. To ascertain the effect of the glass alone in *Air*—it was submitted to trial. It required direct rotation of the analyzer. If any tint was produced, the red was to the right of the minimum (15301).

15276. Gold in Air—also required direct rotation to produce the minimum but it was difficult to procure the tints—when most inclined to warm tints they are to the left of the Minimum; the cold tints are to the right.

15277. The pure gold leaf on ring in Air—polarized ray—inclined—requires right handed rotation of the analyzer to reach minimum—the tints are blue and cold to the left of the minimum. If inclined only a little and the analyzer rotated to minimum light—more inclination brings in the light and more rotation of the analyzer is needed to compensate. The more the gold is inclined the greater is the rotation of the plane of polarization.

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15278. The Jelly in Glass No. C (15087), which is not ruby but turbid, was transferred by warming, etc. into three watch glass[es] to dry, and these were made Nos. 489, 490, 491. The jelly in glass No. CI (15088), which is of a fine ruby and also turbid with gold, was put into other three watch glasses and numbered 492, 493, 494.

15279. Taking the series Ruby glass—ruby jelly—Ruby gut, and so on to ruby fluid, where can we stop? Why is not the cause the same in all?

15280. Filled a large bottle with hydrogen gas made some days ago and which has stood over water. Then introduced a moderate solution of gold, closed it up, agitated well to see if any reduction could be effected and if a ruby could be produced. No appearance of change in 6 hours.

15281. When a particle of phosphorus floats on water, it sends out a film all round it which may be seen near the phosphorus by its high reflective power. It is this which reduces the gold and causes the progressive motion from the center. Phosphorus oxidizes very slowly on the surface of water.

15282. Fine gold leaf heated in a tube in air by a spirit lamp. Wrinkled up by degrees and so became dead in appearance—and by transmitted light became darker than before—but in the part least heated was green. Think that holes opened out, but not enough to account altogether for the change—[illegible] would do a little. Gold leaf in a tube with air over the sand bath wrinkled up—but remained green.

15283. Fine gold leaf in olive oil in tube on the sand bath left for some hours—at temperature below decomposition or boiling, though enough to make the oil brown. It also has wrinkled. *It has lost the green colour.* Poured out contents of the tube on to a glass plate—poured of[f] the oil—inclined the plate—drained off the oil—washed of[f] remaining oil by camphine—and the remaining camphine by Alcohol—then dried it and obtained the gold in a wrinkled leaf—not green by transmitted light—yet apparently continuous—and of a dead yellow colour by reflected light. Examined in the microscope, it was as much a leaf as the original gold leaf and as continuous as it. By pressure with agate a good reflecting surface came out—and by transmitted light the previously pale reddish gray part came out much darker and green.

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15284. Put on two tubes with oil and gold leaf in them into the sand bath as before (15283) and left them at about 1 o'clk. to take the chances of heat. Next day the gold in both had lost its green colour—transferred the pieces to glass plates—drained of[f] the oil—washed them with camphine and then with Alcohol and made them Nos. 495, 496. See (15307).

15285. Attached a piece of fine gold leaf to a lamp glass—lighted the lamp and put the glass on at 12 o'clk. Surrounded it with a paper jacket at a distance $\frac{1}{2}$ inch and closed that at the top to keep in the heat. The heat was enough to char paper. At 1 o'clk.

perhaps a little diminution of green colour: continued the heat until 9 o'clk. P.M. The gold had not lost its green colour and adhered well to the glass.

15286. Agate pressure given upon Gold of No. 180, but with card between the agate and the gold. Still even there the tendency to greening was evident though imperfectly.

15287. Prepared some graduated films of gold by putting particles of phosphorus on to a solution. After 12 hours or more, were good: took them of[f] the solution by *mica* plates. When they were washed and dried, I put gold beater's skin on to the gold film, then plates of smooth paper above and below and plates of card board next, making a bundle and beating it on the anvil with our heaviest hammer. When opened out, there were signs of greening but the gold had rather run up into particles—the effect not very distinct. When the gray gold is first made ruby by heat, then the effect of greening is more perfectly seen because of the contrast of colours.

15288. Assume that the colour of annealed gold is not green but a red gray tint. The compressed state gives green but it does not seem to give polarizing or any other distinction except the green. Is it then in too thin a mass? As some films appear to be too thin to polarize or depolarize?

15289. As to condition of a glass plate in Sulphide of Carbon. When a plate with piece of fine gold leaf on it was in that fluid, the image of a candle flame, though well reflected from the gold, was scarcely sensibly reflected from the glass. Only the outlines of the glass were visible, so nearly is it and the fluid alike.

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15290. Fine gold leaf put on the glass of my lamp; see 15285.

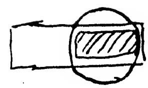
15291. Attached specimens of fine gold leaf to thin plates of glass by water and evaporation, and then when quite dry subjected them to heat by laying them over the top of the glass chimney of an Argand burner, sometimes with the gold and sometimes with the glass upwards, for longer or shorter times. The lowest heat given was enough to scorch paper. The gold adhered well to the glass, being laid on by water.

15292. No. 497—gold upwards—good heat—not long. No. 498—

gold upwards—good heat for 30′—it seemed considerably changed whilst over the lamp flame—the effect seemed slowest at the thicker mottled parts of the leaf. No. 499—gold face downwards: in 12′ the green colour nearly all gone—lowered the flame and annealed a little. No. 500—gold upwards—good heat for 70′. No. 501—Gold upward but a second thin glass plate laid close over it to keep in the heat—left over the flame for 2 hours. No. 502—gold downward—heat good—in 3′ the green colour was gone. No. 503—gold face downwards—left over the heat for 8′, then annealed. No. 504—gold face downwards for about 20′. No. 505—Gold face downwards, over top of spirit lamp flame. See 15308.

15293. Nos. 502 and 503 were put into the throat of the laboratory furnace at night time and left to anneal until the morning.

15294. The great difference when gold is down towards the flame—or up from it—do the products of combustion do any thing, or is it only the difference of heat? The bottom face when gold must become very highly heated—the top face is always being cooled by a current of air inwards from the ends of the plate to the middle, from whence it ascends, for the glass was thus.



15295. Three like portions of fine gold leaf on thin glass plates were put into an olive oil basin bath on the sand bath at $11^{h}\ 35'$—at $11^{h}\ 45'$ the oil becoming brown and pretty hot—but gold not changed—probably time needed. At $12^{h}\ 0'$ oil much changed—still gold green. At $1^{h}\ 25'$ P.M., oil very dark but gold green. At $3^{h}\ 30'$, i.e. after 4 hours heat, put the oil vessel back on the sand bath and covered it well up to cool slowly and anneal the glass (15309). Make the numbers 506, 507, 508.

15296. Arranged the small lamp—the Nicol's polarizing prism looked at from the lamp. Sul. Carbon cell and Nicol prism analyzer as before (   ). Then proceeded to examine the effect of plate of air and of gold (15167), using the double plate No. 426 for that purpose. The air part, being perpendicular to the polarized ray, did nothing—being inclined it depolarized, but a moderate inclination brought on total reflexion. When less inclined than this, an image appeared which required a little direct analyzer rotation to compensate it or reproduce minimum of light. There was very little colour if any—the more the inclination the more

the depolarization, but no colour. When the gold leaf part was in the course of the ray and inclined, there was depolarization, but little inclination of the plate could be allowed. The depolarization was corrected or compensated by direct analyzer rotation. Very little colour if any, except the green of the gold, was developed—if any, the warm tints are to the left of the minimum position and the cool tints to the right.

15297. When a plate of glass is inclined very much in the sulphide of carbon, a darkness comes in over it from the further edge. This is just a consequence of the edge cutting off more and more light and is prior to total reflection.

15298. Resumed the comparison of Gold with Sulphuret of copper (15272–6). Sulphuret of copper No. $\frac{456}{2}$ in S. of carbon—perpendicular to the ray did nothing. When much inclined it brings in a blue image—a little direct rotation of the analyzer darkens it—a little more gives a red image. The red is on the right hand of the minimum and the blue on the left—they are strong colours but a very little way apart. If the sulphuret of copper be inclined in the opposite direction about the vertical axis the same effect results.

15299. Now fine gold, No. 445, being employed; it when inclined brings in also a bluish image and also requires direct rotation of the analyzer for the minimum; though the first image is blue (because of the colour of the gold) yet when the tints are carefully examined, the warm tints are to the left of the minimum and the cold tints to the right, i.e. the reverse of the Sulphuret of copper. Exactly as before (15272, 3).

15300. When a plate of glass was employed—there was either the least possible trace of colour or nothing.

15301. The Glass in Air requires a trace of direct rotation of the analyzer to produce the minimum, and there is no sensible colour (15275).

15302. The Sulphuret of copper and glass No. $\frac{456}{2}$ in Air requires a little righthanded rotation—a little colour appears, the red or warm tints are on the right of the minimum and the cold tints are on the left (15274).

15303. The heated and discoloured golds, 498, 499 (15292) were examined, but the glass not having been sufficiently annealed,

produces so much effect even when perpendicular to the ray as to prevent any observation—but on the whole the gold seems to have no effect.

15304. The gold from the oil tube bath (15284) No. 496, which is not attached to the glass but is loose, gave no effect of depolarization on the ray. But it is very crumpled minutely and cannot be considered as any thing but a corrugated plane—it was not likely to shew any thing.

15305. Brown gold may be the ungreen heated gold. Burnished or polished gold must be the green gold. Though brown gold rubbed by the hand or a card becomes polished, it may and does also become green, i.e. green and brilliant at the same time—former experiments shew this.

15306. Other metals have these states, perhaps all—perhaps something of the same kind may be general to all matter.

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15307. The oil golds, Nos. 495, 496 (15284) which have been heated looked at in my microscope—they are corrugated but there are plenty of thin parts which one can look through—these seem as continuous as a piece of fine gold leaf which was compared with them, but the thinnest part and all parts have lost their green colour and become brown, darker or paler as the parts are thicker or thinner—it does not appear to me as if what remains is green gold separated but rather true annealed gold, which by transmitted light is of a brown colour. The specimens are of a fine dead gold colour by reflected light.

15308. The specimens 497 to 505 (15292)—fine gold heated over gas flame. No. 497, heated short time—tends to lose a little of the warmth of the green tint where most heated—a commencement of action only. No. 498, heated 30′—much more affected—the warm green became blue green, the thinner parts almost colourless—the thicker pimples still green. Much more light therefore passes, but it does not seem to pass by holes. In the Microscope the dark parts are green and thick—they form a mottle—between them are the thinner colourless parts or very light brown; these are not holes—for the holes are very different—they may be reticulations like porous films, but look as if

continuous, as continuous as the thin parts which are green in the unheated gold. The plate has lost a little in reflective power at the place heated. No. 499, heated 12′, gold downwards–green gone to common observatn.–the reflexion very poor and looks as if gold were full of holes–but by a lens these places seem to be continuous but to consist of unreflecting gold–very much light goes through them but it is a pale grey or grey brown light. I believe the gold to be all there, though it seems as if gone, swept away. In the microscope there is a great difference between these places and the lines where the gold is really removed by accident and swept away. Agate pressure restored green and increased reflection, but it is difficult for agate pressure to reach the thinner parts, because the thicker parts stand up and keep of[f] the pressure, and it is these which are greened by the pressure. No. 500. Good heat for 70′–gold upwards. Green colour well gone to common observation. In the microscope was as the last. The green of ordinary beaten gold is chiefly visible in the thinner parts–the other[s] are nearly opaque for a common light–with a high light they also appear green. No. 501, which was covd., is decoloured only along the two edges where it would be hottest–the appearances of the different parts are as in the former specimens. We have had the case of perfect decoloration in covered specimens at the blow pipe (   ). No. 502, in 3′, quite decoloured, gold being downward. Here even the thicker parts are decoloured. Made a streak with a needle point; there must I think be a continuous film of colourless gold on both sides of the streak. Agate pressure brought out the green even in some of the thinnest parts reached. Where the agate had picked off the gold, the microscope shewed the difference between that place and the thin colourless film. No. 503, same thing. No. 504, very greatly changed–in best parts hardly any black portions left to common observation–reflective power almost gone–the light either transmitted or absorbed. In microscope, same as former, except that more thin film and less opaque parts–pressure produced the greening both in thinnest and thicker parts, but the thicker parts took off the pressure from the thinner. I think the gold must be much retracted in the thicker points–and not laid out as in the leaf. The leaf consists perhaps of innumerable scales. Not likely, however,

to be so. No. 505 is affected just as the former, though heated by the spirit lamp flame. There is little or no adhesion of any of these specimens to the glass. When wiped off the latter is quite unchanged.

15309. The gold leaf heated in oil (15295). Nos. 506, 507, 508—were taken out of the cold dark oil—drained—wiped a very little—then put into a glass of camphine to wash off the oil and afterwards into a glass of absolute alcohol to wash off the camphine. The gold on No. 508 was carelessly rubbed off, and it is kept for another reason. Now the green transmitted tint is almost entirely gone from Nos. 506 and 507—so that this treatment has done well and is out of contact with smoke or air—moreover the gold has not contracted or lost shape, but in form looks under the lens and in the microscope as if simply the gold leaf deprived of green colour. They have lost in reflective power, looking pale and brassy, and transmit more light than before. These specimens do much to settling the question whether the contraction of the gold leaf is essentially concerned or not. I think now it is not.

15310. These three plates 506, 507, 508, are covered on both sides with a fine film—by reflection it is metallic and looks red, like copper or titanium, by transmission it is of a green colour, and when two plates are laid over each other, they transmit as green a light as gold. The films seem permanent in air and wiped off from the glass. Under the Microscope it has a mottled appearance and is as if a green film had formed over the whole and then contracted as bitumen does, leaving vein spaces between. Is this carbon or copper? (15320).

15311. Agate pressure on 506 gold brought in the green very well considering the irregular thickness of metal.

15312. Nos. 509 and 510 are glass plates with two thicknesses of gold leaf. It was (509) heated over the gas lamp with gold face downwards for five minutes only—in which time it became permeable to the light of the flame and lost the green colour. No. 510 was the same. No. 511 has three thicknesses of the fine gold leaf—it was over the lamp 10 minutes with the metal downwds. and has like the rest become more permeable to light than before (when it was opaque) and has lost green colour. The lustre in all these heated cases is thin and brassy.

15313. The change of these gold is more easily seen by lamp than by day light because of the smaller proportion of green rays in the former.

15314. Nos. 512, 513 and 514 are fine *gold leaf on mica*. As mica was found to bear the heat requisite to change the gold, so 512 and 513 were put over the lamp and heated for a few minutes. They changed and lost the green colour just as before—so the glass is not the cause of the change.

15315. Fine *Gold leaf on Rock crystal*, No. 515—then heated: the same effect produced (15353).

15316. Fine *Gold leaf on platinum*, No. 516—heated, the gold lost in colour, became pale and the platinum whiteness shone through it—yet not because of holes, but because of the change of the green reflecting gold to pale brown unreflecting gold—a little agate burnishing brought up the bright gold reflexion and hid the platinum again.

15317. Fine *Gold leaf on Silver*, No. 517. Heated. The gold leaf became miserably poor. Almost disappeared as gold, looking quite pale and white. Here the gold appeared to have combined with the silver, for on burnishing the place, though it took a fine polish, it was white like silver with gold—and not the full colour of gold.

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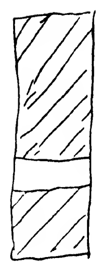
15318. Gold leaf heated until colourless and then submitted to a ray of polarized light as before (   ). Nos. 498, 499, 501, 502, 503, 504 (15308). The glass too much affected by strain. Even two which had been annealed in the throat of the furnace were still depolarizant, so that no observations on the uncoloured gold could be made.

15319. The three specimens which had been heated in oil (15309) and lost green colour there were examined in the ray and of course the S. of Carbon bath. No. 506 when perpendicular to the ray did *not depolarize* either at the glass or gold part. When the gold film was oblique it brought in a red image—and then direct rotation of the analyzer reduced this somewhat and was succeeded by a blue or cold image—the minimum was only a little reduced. When inclined the other side, the effect was the

same. The reduction was not much, but the difference of colours was good. So there is a difference between it and the unheated gold, but perhaps not more than is due to the dissipation of the green colour. If this plate were looked at through a green glass, a stronger minimum would appear by the extinction of the fainter condition of the warm image. The difference may be due to that altogether, or partly to that and partly to a constrained or polarized condition of the gold. No. 507 (15309), being a like preparation, produced the same effect at the gold part: the oblique gold brought in a red image, which direct rotation of the analyzer converted into a blue image.

15320. Now examined the optical effect of the coppery-looking film on these glasses (15310). Employed the clear end of No. 506. Here the bronze film, being vertical to the polarized ray, did nothing. When made oblique, it brought in a fine red image—then direct rotation of the analyzer converted this into a fine blue image, with no sensible minimum between. Effect was the same on all sides of the plate. Wiped half the plate at the place; then the cleared part seemed to have a very little effect of the same kind—but nothing compared to the effect of the bronze coat. The glass was not a good specimen of the coat. No. 507 examined as to the bronze coat on it. Acted just as the former. Wiped part of this glass also—had the same results as with the former glass.



15321. No. 508 has no gold on it, but the bronze film only and that on one side only (15309). A clear place was made across one part of this plate by wiping so as to remove the film. The bronze film placed oblique depolarized, bringing in a red image which by direct rotation of analyzer changed to blue. When the plate was raised so that the clear glass place was in the course of the ray, it gave a feeble pale cold image, but as nothing compared to the film part: the glass has a very little effect due probably to the difference between it and the medium, but the film much; due to its own power and greater difference from the medium.

15322. Examined this red film on No. 508 by acid, testing by ammonia and also ferro pruss. potassa—found it to be *copper*. So put some black oxide of copper into the remaining oil—and also several glass plates—gave them a heat on the sand bath—took

one out after half an hour–signs of the copper film–let the rest have more heat and left to cool in the oil all night.

15323. Provided an apparatus for the deflagration of different metal wires in *hydrogen* between glass plates–and by a power of the large Leyden battery. All succeeded very well and many results were obtained in which the metals as such were spread in a film of fine particles on the plates, and were then examined as to colour and their action on a polarized ray, before any time for oxidation had occurred.

15324. *Copper deflagration* in hydrogen. The deposits reflected a fine red metallic lustre, inclining a little to purple–the light transmitted was a dark green, an olive green, dark but very marked, and general to the finer and coarser parts of the deposit. The reflexion on the metal side was not so regular as that on the glass side; and where the deposit was most abundant, it became dull and of a dead black from disintegration. The contrast between the reflected and transmitted light is good. There was a dust over the whole of the plates–partly the smoke of the deflagration and partly knocked out of the packing cork by the explosion. In the polarized ray and Sul. Carbon, the oblique copper film was as gold–i.e. it brought in a red image, which direct rotation of the analyzer lowered a little to a minimum, and then came on a blue or cold image. The specimens are numbered 518 to 523. Nos. 518, 521 and 522 have been dipped in the Sulphide of Carbon, and so we may expect perhaps a little sulphur action there. The diffuse film on all the glass is browned by this dip.

15325. Agate pressure gives a bright metallic reflexion to parts dull before and makes the transmitted green deeper, i.e. more light is reflected and less transmitted, as with gold; but there is not the same strong change of colour. The effect is I think the same in nature for both cases. See No. 519.

15326. *Palladium wire* deflagrated in hydrogen. Made Nos. 524–529. Reflexion–fine steel grey metallic reflexion, or else where dead black–like the Palladium films by phosphorus. By transmission from Sepia brown to black–the palest are clearly of a warm or brown tint. General cloud on glasses as before. Put No. 524 in the polarized ray. It brought in a red image, as copper–

and then direct rotation of the analyzer gave a fair diminution to the minimum and after that a blue image rising to white, etc. *Agate pressure* on a dull part brought up the full steel grey metallic reflexion—much darkened the transmission and gave it a bluish tint by comparison with the neighbouring brown film. Good result. See No. 524, also 527.

15327. *Platinum wire* deflagrated in hydrogen. Made 530–532. Reflection—white and metallic—transmitted ray brown or warm gray—no other colours. In the polarized ray, the image comes in and it requires direct rotation to bring it to a minimum, but the difference of colour is not sensible. There is good depolarizing effect in selected parts of the films, but still, the development of colour is not sensibly evident. Agate pressure improve[s] the reflexion and darkens the transmission—but it is simply as if it had spread the particles—nevertheless there is more light obstructed when spread than when not.

15328. *Steel Watch hair spring* deflagrated in hydrogen. Made 533–538. Fine metallic reflexion, steel gray or slate colour—the transmitted ray a brown black. It acted on the polarized ray as the other metals, bringing in the light—direct rotation of the analyzer brought on a minimum, but the tints on either side of it were very doubtful. Agate pressure increased the reflexion and a very little darkened the transmission.

15329. *Lead foil strip* deflagrated in hydrogen. Made 539–544. Bright metallic reflexion, white—where dead the reflexion dark brown or black. Transmission, dark smoky brown. In the polarized ray, acted as the other metals—direct revolution of the analyzer gave a minimum, but I could not distinguish colours on the sides of it. Agate pressure brings up the reflected polish—diminishes the transmitted ray and renders its colour bluer. See No. 544. Must remember that all these films are mere scattering and that probably much light untouched by the particles passes through. The lead adheres well to the glass in many parts, being splashed on to it, but when cleared off the glass alone has no effect.

15330. *Tin foil slip* deflagrated in hydrogen. No. 545–550. Reflexion very bright and metallic, white—where dead is brown. Transmission is of various shades of brown, some light, others darker. In the polarized ray—the depolarization occurred and

needed direct rotation of the analyzer to attain a minimum, but the colours on each side of it were doubtful. Agate pressure brought up fine reflexion and darkened the transmission very much. See 545. This is a good illustration of one effect of pressure at least, and it is questionable whether even gold presents any other effect than this.

15331. *Aluminium* wire hammered out and cut up, deflagrated in hydrogen. Nos. 551–554. Reflexion – bright metallic and white except where dead and there dark brown or black. The transmitted ray is dark brown–bluish brown and occasionally in thinner parts orange. In the polarized ray, it brought in a red image–direct rotation of the analyser reduced this to a minimum at a very little distance and after that a cold tint came up. Agate pressure acted a *little* as in former cases–improving reflexion, not much reducing transmission.

15332. *Zinc foil slip* deflagrated in hydrogen. Nos. 555–560. Reflexion brilliant, metallic and white–where dead, brown and black. The transmitted light of a dark smoke colour, blue gray–brown gray and pale brown w[h]ere thin. In the polarized ray, acts as the other, a minimum on the right hand but the colours doubtful. Agate pressure brings up reflexion and tends to make the bluish transmission brownish but does not obstruct much. The lighter deposit wipes off leaving the more metallic film adhering–a dilute acid, as the Sulphuric or hydrochloric, removes that also instantly with evolution of hydrogen and leaves the glass with its full polish: then the glass No. 560–has lost the power of acting on the polarized ray being alone. So glass does not produce these phenomena but the metal.

15333. Antimony powder gummed on to paper; a slip cut off and employed; but the explosion passed *over* it, not *through* it, and the metal was not deflagrated.

15334. Placed three minute globules of phosphorus on water, glass and paper–they remain a long time exposed to air–after four days parts were still there. The surrounding film of phosphorus on water very good and distinct–it is that which reduces the gold solution.

15335. Stretched a fine steel wire across the microscope, illuminated it either directly or by reflected light—brought it into focus—placed a film of gold leaf stretched on a card over a hole between it and the microscope at different distances. I could not find that there was any sensible refraction, i.e. any deformation of the wire when across a light and a dark part of the gold leaf. Moved the latter into various positions, but still no effect on the form of the wire; for the wire employed a fibre of cocoon silk, which however is a very irregular thing, but could detect no refraction deformation.

15336. Perhaps such an effect was hardly to be expected, for considering the matter of the gold uniform, the difference in absolute thickness between the thicker and thinner parts would remove the whole very little way from a plane, i.e. the inclines from thick to thin, etc. would be very small. But besides that, gold leaf is an exceedingly irregular thing and the darker parts are not merely thicknesses but more like accumulated corrugations and heapings.

15337. For a leaf of fine gold in the microscope with my highest power was a very crumpled thing—veins of opaque folds running in all directions and parts across both the thicker and thinner parts—the parts cannot be considered as a mere uniform body spread out irregularly—but are quite a pucker like crumpled paper—the thicker parts seem to be these puckers heaped and beaten together. Yet even the thinnest parts, when compared with real holes, appear very different indeed to them or to any thing like a sieve.

15338. The oil heated and ungreened specimen, No. 506, examined in like manner—shewed no green, and as far more light passed, the structure of the leaf and disposition of the running veiny parts was much better seen. The film is most irregular—still it seems to be continuous, though the palest thinnest parts are now spotted as if running up into aggregations. Whether a very powerful microscope would resolve all these parts into holes I cannot say.

15339. *No.* 500 *heated over the gas lamp* appears wonderfully broken up or eaten away as regards the greened part—so that the dark veins are so numerous and so distin[c]t as to look like moss

in an agate, and yet at cracks or holes the comparitively colourless ground seems quite continuous. Is there absolute retraction of the gold to the extent of forming an infinity of holes through which white light passes, or is there an absolute change in the character of the gold? I think the latter is the case; at any rate, even though division may occur and even if it depend upon division or porosity, it would imply a change of quality, since the retracted gold must be considered as in part transparent and yet transmits no green light.

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15340. *Ruby jellies* and their *effect with water*. The ruby jellies in capsules change colour as they dry. They dry first at the circumferance and then towards the center—so that there is a ring of dry ruby jelly and a central part which is hydrated and projects, being swelled by the water, becoming less and less until it disappears, all being dry. This central part does not become dry at the surface whilst the under part remains moist, but the water from the lower parts travel[s] towards the upper, so that it is apparently of the same degree of moisture throughout. Now in drying, the ruby hydrated part changes in tint and became violet, having much blue in it sometimes; whilst therefore the ring is violet or ruby violet, the center is ruby; the effect is not superficial but in the body of the jelly.

15341. As the jelly takes water again, it returns to its first ruby colour, which colour it loses as it dries, and then the jelly returns to its violet tint again. This again is a remarkable change in the colour of gold. Thus No. 415 is a fine violet, being dry—touched on the surface with a drop of water, there was no instant change of colour, but in a minute or two it began to change and more evidently where the jelly was thinnest, but ultimately through the thickest part; and every part wetted became a fine ruby, like ruby glass. This colour it retained until it was dry again and then it resumed the violet hue. If a line is drawn with water from the center to the circumference, the change in the various parts is easily seen. After a minute or much less, it seems as if upon examination the line of water were still upon the surface, but a touch with the finger or any other delicate object shews that

the water is not on the surface but has entered the jelly, which has a swelled state and convex form. If the moist jelly be covered by a piece of thin microscopic glass so as to adhere, that does not alter the ruby colour, except that the latter remains longer because the jelly is longer in becoming dry. The rapid imbibition of the water is very interesting.

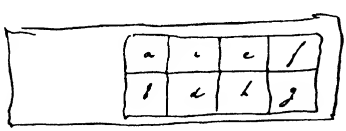
15342. No. 419 is a deep violet jelly, being dry. The same thing happened with it in all parts, the thick and the thin, except that more time, perhaps two or three minutes, were required for the aqueous penetration of the thicker part. On drying–the dry tint returns. Nos. 417 and 418 have salt with them and are of a blue violet colour–but the result was the same; in a few minutes water made them ruby, and a few minutes longer of drying and they became blue violet again.

15343. No. 416 is a faint blue specimen, being dry, and this underwent no change of tint by water–the jelly shewed the effects of absorption, etc. very well, but there was no ruby originally, i.e. in the dry jelly, and no development by the water. No. 411 is a like blue specimen and acts just like 416. No. 412 is the jelly, having a fine golden surface and appearing blue in the middle part and violet at the edges. This did not change by water but here the *gold and colour is superficial.* Making a line with water and then wiping it quickly up with the finger, the gold and the blue colour came away and the jelly beneath was nearly colourless, so that this is not a case of diffused gold but rather a case analogous to a phosphorus film–it represents the latter beautifully.

15344. All this has a beautiful relation to the ruby colour of particles of finest gold diffused through water and even through glass–the change of the ruby and violet to and fro is beautiful.

15345*. The heated gold No. 504, which has been more heated in one part than another, so as to have both green gold and colourless gold present, was divided into eight parts, being cleared of gold along the lines marked and then a line of soft cement drawn between each. The plate was then placed horizontally in an atmosphere of chlorine and in 12 hours the gold was all converted into chloride. Being examined, the film of concentrated chloride of gold on *a* and *b*, which were left as green gold, did not appear

* [15345]



to be more than that on *c*, *d*, *e*, *f* and *g* (*h* gold had been wiped away), the three first of which had been made colourless and the two last nearly so. Leaving the plate exposed to the air so that the chloride should crystallize–long needle form crystals shot across the spaces–the crop being as abundant from the colourless gold spaces as from those left green.

15346. *Copper films*, 15322. Took the plates out of the oil–drained them–washed them by steeping in Camphine–then in Alcohol–and obtained plates beautifully coated with copper. Numbered some with films on one side only: 561, 562, 563, 564, 565 are such. The metallic reflexion and colour is beautiful even on both sides. The transmitted colour is a fine green, not so yellow as that of gold, but No. 564 is equally if not more beautiful and is as yet without pressure. Nos. 566 and 567 have films composed of very coarse particles and yet a lens here shews the tendency to green black. Nos. 568, 569 and 570 have films on both sides.

15347. Placed the copper films in S. of Carbon in the polarized ray (   ). No. 561 an excellent single film–brought in a red image–direct rotation of the analyzer diminished the light and then brought in blue image–but the red produced at first is not the most intense red, for a little reverse rotation of the analyzer makes it more red. Certainly the minimum is a little to the right, i.e. requires a little direct rotation. No. 565 is also a single film of copper and gave the same results, but the minimum was not so distinct as with 561. Placed these two back to back at once so as to have a compound plate in the medium of copper, Glass, S. Carbon, Glass and copper–expecting that the three intermediate plates would go for mediums and the coppers act as two plates. The effect was not better than for 561 alone. 565 is a comparitively thin film. No. 569 is glass with a copper film on each side, both being good. The results were the same as before and not better than 561 alone.

15348. The Sulphur in the S. Carbon dissolved seems to act on the copper quickly, changing it. Pressure of the agate renders the copper tint greener I think–it has not the opportunity of increasing the reflexion, for that is nearly perfect as it is.

15349. Examined the Ten Jellies (15206), it being now 16 days since they were looked at. They are in wide mouth small bottles, the mouths covered with bibulous paper. Warmed them–transferred them to capsules and when dry numbered them as follows:

No. 1. Just as before and the jelly tremulous and sound. Nos. 571 and 572.

No. 2. Ruby as before, but running to decay–becoming fluid–remained fluid till dry. No. 573, 574. R ∪ P[1]

No. 3. Colourless as before–sound and tremulous–dried white, much salt–no colour, very poor. Nos. 575, 576, 577.

No. 4. As before–sound and tremulous–dried thin blue–gold down in brown deposit. Nos. 578, 579, 580. Rubifies by water. P

No. 5. As before–sound and tremulous. Nos. 581, 582, 583. Blue.

No. 6. As before–but mouldy and becoming fluid–fine ruby. Nos. 584, 585, 586. R ∪ P

No. 7. As before–sound and tremulous–blue. Nos. 587, 588, 589. Rubifies by water. R

No. 8. As before–but becoming fluid and mouldy–fine ruby–salt. Nos. 590, 591, 592. R ∪ P

No. 9. As before–sound and tremulous–very poor. Nos. 593, 594, 595.

No. 10. As before–beginning to mould and decay–very poor. Nos. 596, 597, 598. There was much phosphorus at the bottom–hence smell, etc. ∪ P

All those with phos. except No. 4 have begun to decay and liquify.

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15350. Put all the golds on to glass plates and all the glass plates into a bath of pure oil containing no copper, and heated on the sand bath from 7 o'clk. until 11 o'clk. Almost all the golds came loose from the glass plates, but I had not been careful to heat the plates a little in the air first to drive off all moisture and air. The green of the fine and deep golds had *not disappeared*–

[1] The marginal notes are in pencil in the MS.

whether this is because there had not been heat enough I do not know as yet.

15351. The former ungreened plates, Nos. 506 and 507, had been heated in a bath of oil containing a very little copper, and a film of copper was no doubt over the gold (15310). To see if this could conceal the green, I placed pure Mur. acid on the surface of No. 506. It dissolved a little copper and also a little gold, but the film of gold left was as before, not green. Then washing the gold first, pure strong Nitric acid was put on to it and heated to remove all copper. The gold leaf, which remained untouched, was *not green*, but still as before. The place which had been made green by pressure remained green all this time.

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15352. Fine Gold leaf into pure oil in a tube, well heated on the sand bath for some hours. It shrivelled up and contracted much, clinging together–being opened out in some degree upon glass and washed with camphine and alcohol, it appeared as a dead contracted crumpled film. When examined by a lens, many parts seemed translucent and of a pale brown–no part seemed green. In the microscope also–the same result. So the effect of heat and pure oil is as before. No. 599.

15353. Fine gold leaf was laid upon plates of Rock crystal, gradually dried, warmed and heated over the gas flame with the gold leaf downwards (15292, etc.) for 2, 3 or 4 minutes until the change of the gold came on more or less (15315). Numbered 600, 601, 602. The first not changed–the other two changed in the heated places. By lens and my microscope the thin places change first but do not seem to have disappeared as films–the thicker parts change by more heat. I cannot think the disappearance of the green is due to holes formed or that the gold is not there. Agate pressure restores the green beautifully and here I can reach the thinnest places, and there the green is beautifully restored. There is no doubt that the gold which there was green, then became colourless and then green again, is the same gold which has never left its place; but whether the thin film is in holes after heating, my microscope will not shew. Try De la Rue's.

15354. The green can hardly be an effect of foldings because it is most evident in the thinnest parts—disappears there first—but *reappears* there as well or better than in thicker parts.

15355. The agate pressure in restoring green restores also the polished reflecting surface.

15356. Various golds and a silver laid upon crown glass, put into a little muffle and that placed in the throat of our furnace for 4 hours perhaps, the heat never rising to redness but being higher considerably than the sand bath—the muffle was closed, all but the mouth, which had a loose stopper so that sulphurous fumes, etc. from the coals had an impeded access. When they were taken out next morning, having annealed all night—the glasses were found quite undisturbed in form but covered with a white dead film which wiped off perfectly. Whether it is deposit from the smoke, etc. or action on the glass, I do not know as yet. The gold seems covered with the same coat, but as it is quite dead, I cannot tell as yet. Must try rock crystal. All the golds look alike. All trace of green is gone and the gold is by transmission a pale gray film appearing by the lens and by my microscope to have the same mechanical structure as the original gold leaf. The reflected appearance is a pale brown, with no metallic lustre about it, except that, under the lens, points of gold lustre might be seen here and there, especially on the glass side—the touch of a card takes the gold off—the light draught of a knife point also removes it, but at the same time burnishes it up to bright gold lustre. Pressure or tapping with the Rock crystal convex brighten the metal surface but not the glass surface much—as if the latter had been injured by the film, or had the film under the gold. As to the devel[op]ment of green, it appeared—but very imperfectly; nothing like what happens on rock crystal. But a *burnish streak* with the rock crystal, though it tore up part of the gold, left fine green traces in the disturbed metal and shews well what such disturbance as rubs the gold particles together can do. One good hard streak with the convex rock crystal is

best—continued burnishing tears up the gold. No. 603, 604, are fine gold. No. 605 deep gold (14485). Nos. 606 and 607 new gold. No. 608, Green gold (14485). No. 609, Silver leaf. As No. 608 was green gold, it no doubt gave a purple tint before

heating—now it gives a green colour by the rock crystal rub—as if the silver had been dissipated (15359).

15357. No. 609, Silver leaf, has disappd. as silver. The parts covered by it are more transparent and bright than the uncovered glass—places are left with a yellow coat—but all over this coat is contracting and drawing up into globules which in many parts are abundantly visible to the eye, forming little plano convex colourless lenses—hard and adhering well to the glass even against washing—the finer ones give way—but all the silver is resolved into these globules, which must be comparitively very fluid.

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15358. Solution of Chloride of gold on plate of rock crystal—evaporated and held with gold downwards over spirit lamp to reduce the gold. All can wipe off easily by card. Much at the edges is in a fine ruby film and some parts blue—so that colours do not depend on the presence of glass. Rock crystal pressure produces fine green in the ruby and elsewhere. Made No. 610.

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15359. Laid fine gold on plates of rock crystal instead of glass and heated them as before (15356). Next morning took them out. The rock crystal was bright and untouched, so no action on it like that on glass—it was cracked in every direction. The gold upon it had lost all trace of green colour and was now a dull dead film, very translucent—it wiped with the least touch from the rock crystal. In my microscope I could not distinguish any separation into globules, nor by the lens. The transmitted tint was pale brown. Rock crystal pressure brought out full lustre—the transmission of light was very greatly diminished, perhaps a tenth only passing—and green tints appeared. The effect on the gold looks like a retraction into gold dew in these extreme cases of heat. Must try less heat, and silver also on rock crystal. Made these No. 611.

15360. In relation to 15277, compared gold leaf, glass, air, etc. etc. together with a constant position of polarizer and analyzer and axis of inclination. Position of polarizer as looked at was[1].

[1] See the diagram.

Gold leaf on copper ring *in Air.* Inclined it, depolarized the ray; required *direct rotation* to go to a *Minimum*—and there were tints appearing on each side of the minimum—right hand or *direct* rotation bringing in *blue or cool tints* and *reverse rotation* bringing in *warm tints.*

15361. Crown glass in Air, as gold leaf being inclined, i.e. it required direct rotation to bring in the minimum, but there were no sensible colours on either side of this minimum.

15362. The Glass in Sul. Carbon—inclined: no sensible action. The glass and green gold leaf in S. Carbon—inclined—depolarized—required direct rotation to reach a minimum. Warm tints on the left of this minimum and cold tints on the right or with direct rotation. As before.

15363. Tried some of the gold deprived of green colour by heat. No. 507, heated in oil—inclined in S. of Carbon: brings in a red image, feeble—direct rotation of the analyzer, goes to a minimum and then on to blue image. So is as gold in the direction—but it is feebler I think—that the red image is more distinct is because the gold is now colourless or nearly so and more light passes also. No. 506, the corresponding specimen of heated gold, did the same.

15364. No. 605 is Gold on glass from the Muffle (   ); it is quite dead and lustreless, and almost colourless. It has no effect on the polarized ray—being without continuous surface, i.e. dead; perhaps it was not to be expected; but I think the specimen looks quite like a dew of gold, exceedingly fine but yet not as the deflagrations, etc.

15365. No. 601. Is gold leaf heated on rock crystal, the specimen being partly un-greened. The rock crystal is too powerful in its action and prevents any observation of the gold effect.

15366. Gold leaf in Air and polarized ray. Ascertained the existence of the four alternate quadrants (15277). Whether the quadrant was such as to require direct or reverse rotation of the analyzer to gain the minimum, the warm tints came on before the minimum and the cold tints after it.

15367. Polarization of a common ray by metallic film, the Nicoll polarizer being removed. Fine gold leaf on Glass in Sul. Carbon polarized the beam—but very poorly. No. 506, or decoloured gold on Glass in S. of Carbon, gave a beam differing in colour

as the analyzer, but offering no extinction of the ray. No. 605, or gold heated in Muffle and quite lustreless, produced no sensible effect of polarization, as it before presented no sensible depolarizing effect.

15368. Three golds and a silver—leaves on rock crystal plates in the muffle and furnace throat for 2 hours only and at more moderate heat. Taken out and cooled. The crystal plates were not shattered. The golds were partially affected, No. 612 most—the other two less and purpose to reheat them; 613, 614. The Silver has become astonishingly translucent; it looks like a piece of paper or thin mother of pearl; pressure does not make it opaque. It has a white mother of pearl lustre but I do not think it can be in the metallic state. Put more in to the muffle in a glass tube perhaps, or on rock crystal, or simply in. Made 615.

15369. When Gold leaf is looked [at] closely it shews the mottle, etc. When the heated gold leaf without green colour, as 605, is looked at direct, it seems to have lost this mottle, but when viewed by the lens and an oblique light, they are seen again; but the thicker parts are very translucent.

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15370. Placed the golds on R. crystal, Nos. 613, 614, in the Muffle again and heated them moderately for three hours. Were more affected. Keep 613. Reheat 614. Some silver wire in and about glass had also been introduced—rendered dead but not otherwise strikingly affected—reheat it. Some silver leaf, nearly three leaves, rolled up and put into a hard glass tube and that into the muffle in the hottest end. Now very greatly changed in appearance. Scarcely any part has metallic lustre, only that which, being on the bottom of the muffle, had least heat and less access of the air or atmosphere in the chimney than other parts. The leaves have sunk together into a loose white pearly heap, looking like an aggregation of chloride of silver films, and quite unlike silver leaf. Heated to redness in a glass tube—it contracted—adhered to the glass but did not otherwise change. Placed on platinum and heated by blow pipe on the surface, it at bright red heat fuzed and ran together. No. 616 and 617 are specimens as they came from the muffle. No. 618 are specimens heated and partly fuzed on the

surface. In Sulphide of Carbon and in Absolute Alcohol it looks much as in air. No. 619 is the silver and Alcohol to give particles for the microscope. Strong Ammonia by standing on it dissolved a very little silver—no signs of any oxide. Dilute N.A. standing on it dissolved nothing. Strong N.A. dissolved it cold and produced nitrate of silver.

15371. When put between Zinc and platinum it was found to be a good conductor by the tongue. It presses together and assumes the full appearance of silver. Agate pressure brighten[s] the reflexion to full metallic and darkens it greatly as to the transmitted ray—it has every appearance of silver, greatly divided.

17 NOVR. 1856.

15372. Muffle experiment ( ). No. 614 gold in again; came out quite decoloured. Crystal not injured—is a good case of gold and heat.

15373. Silver leaf in tube as before (15370). Six leaves of silver weighing 4·7 grains in a Glass tube weighing 253·3 grains. When taken out there was no change in weight—the leaves had fallen together but were changed just as before—the light of a candle could pass through places where there must have been from 40 to 50 thicknesses of leaf silver. Looked at in my microscope, I incline to think it is just a case of altered arrangement—but it is wonderful how the light creeps through. Made tube and Silver leaf No. 620.

18 NOVR. 1856.

15374. Eight leaves of silver in 16 thicknesses in a roll—in a tube—in the muffle. No. 620*a*.

20 NOVR. 1856.

15375. Four fine gold leaf on Rock crystal, numbered 621, 622, 623, 624. Four silver leaf on Rock crystal, numbered 625, 626, 627, 628. All these heated at once in the muffle. The silver, which were intermingled, have gone sooner than the golds, requiring only a lower temperature. They are all well changed. In my microscope they look quite like cases of contraction—there are curious crossing veins, etc. but the visible spaces between the particles of gold

seem as light as clear spaces, i.e. spaces cleared on purpose. Here the effect is gone to the extreme, and the extreme result of contraction is obtained. The Golds are more or less decoloured, Nos. 623 and 624 very well. They have not been overheated and the film of gold seems to be every where; a scrape with a needle point shews that in my microscope. Besides which, Rock crystal pressure greens it well at the very thinnest and most transparent part, shewing gold there. More heat would I believe make this gold retract as the silver has retracted, but I do not think that has yet happened in these moderately heated specimens.

24 NOVR. 1856.

15376. I have a quantity of ruby jelly made variously with salt, Sul. ammonia, etc.—have put some on flat glasses and other in watch glasses—as they dried the salt was excluded, crystallizing out. Soaking these in water, the jelly swelled up but did not dissolve (as jelly does not dissolve thus), but the salts soaked out then removed the salts and saline water rewarmed the jellies and let them cool, coagulate and dry. All these jellies are marked thus ϕ. Some have not the salts removed and there they are seen crystallized in the Jellies.

15377. Some of this ruby jelly dissolved in warm water—and then Proto chlo. tin in solution, Phosphorus in ether, Phosphorus in Sul. carbon, added to portions of it and left—they produced no increase of tint or change of colour. All the gold seemed disposed of either as ruby or in combination. The surface of some of the portions of jelly in capsules (15376) became covered with golden film here and there as if there was gold yet to reduce—the tests did not find such. Neither did dilute Sul. Acid—or Hydrochloric acid—or Nitric acid—change the tint from ruby. Sol. of chlorine did not change the tint but gradually dissolved it. The gold seems as if it might be in association with the jelly, I mean the ruby gold as such.

15378. Diluted some of this red fluid considerably to see if by standing the ruby would fall. Marked it CVIII.

15379. Made a Ruby jelly thus. Sol. Gold and Phos. in S.C., then jelly with water warmed and some salt—mixed both; they

mixed well and gradually gave a good ruby jelly. It and its preparations are marked ✱. When first made, it was in two capsules, having been evaporated partly in them. These were both soaked for some time in water, but probably not long enough to take all the salt out. In fact four flat plates on which some of ✱ was put to evaporate gave results with salt crystals.

15380. Some of ✱ diluted—then N.A. added and heated. The ruby became pale and the gold dissolved—naturally, for Chlo. sodium was present (15386)—the gold in solution was shewn by a gold film forming round a spot on the surface, probably phosphorus—also by Sol. Phos. in Ether—also by proto chlo. tin. A black powder was left which seemed to be a compd. of gold and jelly but doubtful—it did not dissolve in N.M.Acid until it had been heated, when it charred and then dissolved—but little signs of gold.

15381. Looked over the various ruby fluids to see what changes had happened since ( ). Tried all those of good colour, deep or pale, with the sun's rays and a lens to see if uniform as they looked; they were still fluids, holding particles in suspension.

XXVII (14906)—Ruby fluid—very pale. Cone of rays.
XXVII A (14906)—Fluid very pale—amethystine deposit. Cone of rays.
XXVIII—pale blue fluid—uniform—blue deposit—cone of rays.
XXX A (14907)—pale amethystine ruby fluid—cone of rays.
XXX B (14908)—deep ruby fluid—cone of rays—ruby deposit.
XXX D (14908)—good ruby fluid—ruby deposit.
✓ XXX E (15001)—fluid colourless—deposit at bottom—brown and blue—poor.
✓ XXXIII (14865)—fluid colourless—deposit at bottom—shaken up, blue.
Good[1] XXXV (14909)—clear ruby fluid—cone of rays.
XXXV A (14978)—fluid colourless and very poor deposit—dismissed.
XLII (15063)—blue mould at bottom—dismissed.
XLII A (14910)—colourless fluid—little blue deposit—dismissed.
XLIV (14925)—mouldy—blue adhering deposit.
XLIV—colourless fluid—purple deposit.
LII (14911)—fluid colourless—amethystine film on glass.
✓ LII A 1—colourless fluid—little blue deposit.
✓ LII A 2—colourless fluid—coarse brown deposit.
LV *a* (14912)—colourless fluid—little blue deposit—dismissed.
G LVIII (14902)—fine uniform ruby fluid—cone of rays—deposit ruby.
G ✓ LVIII *c* (14986[2])—good blue deposit.

[1] Ticks and notes in the margin are in pencil in the MS. [2] ? 14896.

√ LVIII *d* (14901)—fine uniform violet fluid—cone of rays.
LX *b* (14913)—very little blue brown deposit—dismissed.
LXI (15028)—good uniform ruby fluid—no deposit—cone of rays.
√ LXII (15086)—deposit amethystine and blue.
√ LXIII (14918)—colourless fluid—heavy black deposit.
LXIV (15087[1])—uniform amethyst fluid—cone of rays—little flocculent deposit.
G LXV (15028)—ruby fluid—cone of rays—little fine ruby deposit.
G LXV *a* (14936)—pale amethyst fluid—ruby deposit.
G LXVII (15057)—uniform deep ruby fluid—no deposit—cone of rays.
√ LXVIII (14980)—clear fluid—deep blue deposit—mouldy.
G √ LXIX (14956)—good ruby fluid—little ruby deposit—cone of rays.
LXXI (14993)—nothing hardly—dismissed.
LXXI A (14993)—dark amethyst fluid—cone of rays—little blue deposit.
LXXI B (14993)—little blue deposit—dismissed.
LXXII (14994)—colourless fluid—no deposit—bottle stained ruby.
√ LXXIII (14980)—colourless fluid—little blue deposit—much blue mould.
LXXIV (14963)—deposit insensible—dismissed.
√ LXXVI (14995)—colourless fluid—dark blue deposit.
G LXXVII (14995)—colourless fluid—fine dark blue deposit.
√ LXXVIII (14967)—ruby tint gone—mercury clean—glass stained.
LXXIX (14996)—colourless fluid—ruby violet sediment.
LXXX (14982)—close brown sediment.
G LXXXI (15060)—fine ruby fluid—ruby deposit—cone of rays.
√ LXXXI A (15060)—colourless fluid—ordinary flocculent heavy deposit.
√ LXXXI B (15086)—little blue deposit.
√ LXXXII (14985)—Amethyst fluid and amethyst deposit.
G LXXXIII (15086)—fine deep fluid—cone of rays—ruby and blue deposit.
G LXXXIII B (15047[2])—amethystine fluid—good blue deposit.
G LXXXIII C (15135)—fine ruby deposit.
LXXXIII D (15135)—blue deposit adhering to bottom.
√ LXXXIV (15018)—no amalgamation as yet.
√ LXXXV (15032)—colourless fluid—dark blue deposit.
√ LXXXVI (15061[3])—colourless fluid, very mouldy—blue mould and deposit.
√ LXXXVI A (15061)—amethystine deposit—mouldy.
√ LXXXVII A (15015)—Brown deposit—is platinum.
√ XCI (15020)—fluid very dark, like pale ink—white deposit below, phosphorus.
XCIII (15029)—colourless fluid—blue deposit.
XCIV (15057)—very mouldy—mouldy blue deposit.
XCV (15066)—pale blue fluid—flocculent blue deposit.
XCVI (15066)—flocculent blue precipitate.

[1] ? 15057. [2] $15047\frac{1}{2}$. [3] ? 15014.

15382. The jelly marked thus ✱, of 15379, has been soaking well in water—changed once or twice at different times—the last water was to-day poured off and examined—it has no colour nor ever has had during the washing—neither is there any salt or M.A. or any thing that can be found by Nit. Silver, proto chlo. tin or potassa—in fact it is pure and I conclude the Jelly is pure also.
15383. This jelly was then warmed and about thrice its volume of pure water added; it formed an excellent ruby fluid. It was perfectly neutral, neither acid or alkali or salt or any thing of that kind being present. Different portions had the following agents added to them—

1. *Nit. silver*—no precipitation or change of colour either immed. or after 48 hours.
2. *Proto chlo. tin*— . Do. . . . . Do. . . . .
3. *Potassa*— . . . Do. . . . . Do. . . . .
4. *Lime water*— . . Do. . . . . Do. . . . .
5. *Phos. in Ether*— . Do. . . . . Do. . . . .
6. *Phos. in Sul. Carbon*—Do. . . . . Do. . . . .
7. *Sol. Sul. hy.*— . . Do. . . . . Do. . . . .
8. *Dil. M. Acid*— . Do. . . . . Do. . . . .
9. *Dil. Nitric acid*— . Do. . . . . Do. . . . .
10. *Dil. Sulc. acid*— . Do. . . . . Do. . . . .

Continued so until the 13th Decr. and were then dismissed.
15384. Some of this fluid (15383) was boiled for some hours—no change. It was boiled even to dryness but remained as ruby as before. Water was added: some absorbed and the jelly softened—but though ruby and soft, it did not now dissolve in warm or hot water. The Jelly was changed but not the ruby. The water poured off and strong N. Acid added and heated—then the jelly dissolved—the fluid gradually became amethystine and by further heat and evaporation left a blue smooth substance—water added dissolved the organic matter and then heavy blue gold of the ordinary character settled, having little appearance.
15385. Some of the fluid (15383) boiled over zinc filings for some time and even to dryness—no change was caused in the ruby substance or its characters, except that, being evaporated to dryness, when redissolved the ruby had become amethystine.
15386. Referring to a former case of solution by Nitric acid (15380) and now acting by N.A. and heat, there was no like

action—no solution of gold or disappearance of colour—for now there is no chloride left in the ruby fluid and nitric acid alone cannot dissolve the gold. Its effect is only to change the colour to amethystine and then to blue and then it evaporates. When water is added to dissolve the altered organic matter, the gold is left as the usual dense dark or blue deposit.

15387. Some of the washed ruby jelly was put with water into a flask in the warm air cupbord to decay and change.

15388. A piece of this jelly * (15382) has been drying for some time—it is very dense but not yet dry. It looks black, so deep is its colour. Mark it .

15389. The non action of the tests (15383) can I think only be accounted for on the supposition that the ruby substance is metallic gold and not an oxide. To compare them with results with pure sol. chlo. gold, a weak solution of the latter was tested by the following substances. A portion

with *proto chlo. tin*—changed at once—in 24 hours deep brown.
with *caustic potassa*—colourless at once and after 24 hours; then proto chlo. tin gave a dense pale brown precipitate—because of Alkali.
with *lime water*—colourless at once and after 24 hours—then proto chlo. tin gave deep brown precipitate.
with *sol. Sul. hydrogen*—action at once—in 24 hours dark brown.
with *Phos. in ether*—action at once—in 24 hours fine ruby.
with *Phos. in S. Carbon*—action slowly—in 24 hours fine ruby.
with *Zinc filings*—gold separated—in 24 hours none left in solution.

Unchanged after a week.

15390. Another portion of ruby jelly which had both common salt and chloride Amm. in its solution has been left soaking for 8 or 10 days—the water was changed yesterday and to-day. The first old washing water had no colour—contained no gold by Proto chlo. tin—Nit. silver gave a precipitate of chloride which quickly became yellowish, and then a drop or two of N.A. caused no resolution but made the precipitate of a pale warm brown, as if a trace of gold might be there. When N.A. was added first and then the N. of silver—the precipitate remained white—it was simply chloride of silver. A little of this washing water evaporated in a capsule—gave an animal residue which with water gave [illegible] smell and brown solution but no signs of gold. A little dissolved organic matter here. The second washing water gave

scarcely a trace of chloride or other substance. The third washing water was quite pure.

15391. This jelly with a little warmth dissolved in its own water and gave deep ruby fluid—a portion of it with its volume of pure strong N. Acid—underwent no change in colour in half an hour; being warmed, there seemed to separate a ruby deposit, not heavy and perhaps a compound with jelly or some derivative—warmed more and then diluted—it remained ruby and though opalescent, a deposition did not take place in an hour.

15392. A portion of it (15391) with its volume of strong pure sol. of hydrochloric acid—did not change in colour in half an hour—being heated and evaporated, it slowly became amethystine but threw down nothing—when dry it was amethystine and organic; water added dissolved it, giving an amethystine liquid with a little of blue gold particles at the bottom. A little Nitric acid being added rapidly rendered the fluid blue, and by means of heat and the M.A. still present, dissolved the ruby and form[ed] a good solution of gold.

15393. A portion of it (15391) with its volume of strong Sulc. acid—did not change colour in half an hour—a little heat soon changed the ruby to blue, feeble like the denser gold precipitates—on adding water, the blue gold sank to the bottom leaving a colourless fluid above.

15394. All consists with the idea that the ruby is metallic gold.

15395. A portion of the ruby jelly (15391) in a tube with solution of Sul. Hydrogen—warmed—but remained ruby—there was no alteration of the gold.

15396. A portion of the ruby jelly (15391) put into a tube with filings of zinc, and then a few drops of dilute Sulc. acid added so as to act on the zinc and evolve hydrogen—the ruby tint did not change. A like preparation in which hydrochloric acid was employed evolved hydrogen but the ruby did not change. These were left. A week after, there was no change in these glasses (15393, 5, 6).

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15397. Jelly left in a flask decays, moulds, liquefies and, especially at the upper surface, becomes bad in smell.

15398. Put some of the four deposits LVIII C, LXII, LXXXIII B and LXXXIII C into jelly in watch glasses—marked them so and left them to dry.

LVIII C.	When moist is	amethystine,	when dry	blue.
LXII.	„ „	amethyst	„	blue.
LXXXIII B.	„ „	blue	„	blue.
LXXXIII C.	„ „	ruby violet	„	blue.

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15399. Gallic acid—dissolved in water—a weak solution of De la Rue's Gold—a little of the Gallic acid added to it: turned red at the bottom at once and then change went on quickly and the gold came down a fine blue as with sul. iron—or green blue; by standing 24 hours all the gold separated—liquid poured off—deposit washed with water—was the dense blue precipitate. A portion of the solution of gold made much weaker and very little Gallic acid added—the action only after a time—the fluid became ruby and did not settle in 24 hours—then more gallic acid—no immediate change.

15400. Some jelly ruby fluid with some of this gallic acid underwent no change of colour in 24 or 72 hours.

15401. Pyrogallic acid dissolved in water—when solution of gold was of first strength, the pyrogallic acid made it at once ruby and in 24 hours a dark flocculent adhering precipitate of gold was produced. With the more dilute solution of gold a ruby was obtained, remaining ruby for 24 hours and turbid by reflected light. This divided into two portions; one left—the other had more pyrogallic acid added—it changed at once to darker amethystine colour and was left 3 days—it still remained amethystine and slowly settling.

15402. The ruby jelly with this pyrogallic acid underwent no change of tint in several days.

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15403. Made more deflagrations of gold wire in hydrogen—letting the wire soak in the hydrogen for some hours beforehand. Ruby, green, etc. just as the former specimens (14835–41). Made these specimens Nos. 629–635.

15404. *Time in Magnetism.* 13942–64, 93, 4231–42. Also Exp. Researches, Par. 2166 and vol. II, pp. 191, 195.

15405. Began to prepare Galvanometers for indications of time (13947, 54). Have obtained some fine steel brooches—they are square, hard-polished, about $\frac{1}{60}$ of an inch square and an inch or more in length. Magnetised one powerfully—broke of[f] a piece about 0·1 of an inch in length and suspended it by a filament of cocoon silk about $4\frac{1}{2}$ inches long—it was a good magnet—it vibrated quickly and that is good—its momentum being very small and its magnetic force comparitively strong. It felt both the earth's force and also the torsion force of the suspension: the latter is large in proportion for these small needles. To compare them—the suspension filament being $4\frac{1}{2}$ inches—its point of support was turned until the needle was deflected so far on one side as to be just on the point of upsetting; then the upper end of the suspending filament was turned reversely, and it required about $2\frac{1}{2}$ revolutions to carry the needle round to the upsetting point on the other side, i.e. through the 180°. So this measure[s] comparitively the torsion force of the suspending thread. I do not think that this will be an objection—for it is the *first* movement of a quick moving reflector needle that is required.

15406. Having *two* such little magnets, they were attached astatically by soft cement to a piece of fine copper wire about $1\frac{1}{2}$ inches long and suspended by cocoon silk; but now the earth's directive force was so small that the torsion force of the filament over-ruled all, and the system turned with every turn of the suspending thread. The mass of this compound needle was much greater than the former and therefore its movement and vibration far slower. This would be a disadvantage—the smaller the mass to be displaced by the magneto-electric induction current the better. The suspending filament was 4 inches long.

15407. The single needle (15405) was so attached as to have one of its plane reflecting faces vertical. Employing a taper light as a source of a ray, and then bringing the eye into the right position, it was found that the steel surface was an excellent

reflector. When the taper was 7 feet from the needle and the eye 15 feet from it, the reflected image was very bright and good, especially when seen by the opera glass. Any vibration of the needle sent the image out of sight, so that the tremor of the floor was quite sufficient. So far I think the principle will do.

15408. The taper being brought up close to the needle, gave (of course) a reflected image not disappearing so suddenly. The more distant the taper the more parallel are the rays and the more sudden is the disappearance. An Electric light at some distance will be the best. Also the farther off the eye is, the more sudden and sharp will be the disappearance, and that is what I want.

15409. Observation as to appearance of reflected images in moving mirror. Moon shining, and when seen in the mirror appearing near the reflected image of a dark chimney; on moving the mirror the line of light of the moon[s] travelling image was very beautiful and manifest, but the chimney image scarcely seemed to leave its place. The moon seemed to go up to and receed from the chimney: yet their angular separation must have been the same all the time. Again—at Brighton, 16 Mar. 1857, regarded a near and a distant gas light in the moving mirror at same time: the distant gas light seemed to obey the motion of the mirror quickest; its image was moving back whilst the image of the nearer and brighter light was still advancing—the two did not seem to describe like parts of their paths at the same moments. The effects are probably due to the degree of impression on the eye and the consequent duration of that impression—but they require to be known and guarded against. Must watch to see that they do not interfere with my intended results.

15410*. Suppose A and B connected together with the galvanometer at G, all in one circuit—and P an inducing current so arranged that the current induced in A should deflect the galvanometer one way, and that induced in B by the same act (as the establishing current in P), should deflect it the other way. Then if time were necessary, the needle should be moved for an instant and then stopped. Could such an effect be observed by making the light distant and looking into the needle mirror? The light, at first a fixture, should then describe a short line and then stop suddenly

* [15410]

and afterwds. swing slowly back by the torsion. It would require however that the induction force in A and B should be equal; would several spirals at B produce that result? Would have to work out and arrange that first.

15411. If one wants to make the induction on a distant spiral equal to that in a near one, is that to be done by making the distant spiral of thicker wire, or of more convolutions of the same wire, or by both?

15412. I have two galvanometer coils prepared in which the magnetic reflectors (15405) can be hung by cocoon silk $5\frac{1}{2}$ inches long. One coil, B, is of wire $\frac{3}{20}$ of an *inch thick* and it contains 38 inches in length in 8 convolutions. The other coil, A, is of thinner wire and greater length, being 0·033 of an inch in diameter and 43 feet in length, there being probably 120 convolutions or more in it (15450, 1, 2).

15413. Thick wire coil B (15412) and one magnetic needle (15407) connected with small zinc and platina plates by fine wires—the plates to be excited in due time by the tongue. The needle, observed merely by the eye, did not seem to move on making or breaking contact with the tongue—the taste on the tongue came on, shewing conduction through. Then a candle was placed 42 inches from the needle and its reflected image observed by the eye 14 inches off. Now when contact was made the image was seen to move, and again on breaking contact—all right.

15414. The longer thin wire coil A (15412) with its single needle (15407) was connected with like zinc and platinum plates. On observing the needle directly, it was seen to move quickly on making contact—then to settle oblique—then to move the contrary way on breaking contact. On observing the reflected image as before (15413), the apparent displacement was quick and large both at making and breaking contact. This arrangement does exceedingly well.

15415. Now varied the source of the galvanometer current, using an *indirect current* for the purpose. I have a double helix, i.e. two wires wound one over the other on a tube 0·72 of inch diameter. The wire is about $\frac{1}{35}$ of an inch in thickness. The inner, A, is 37·3 feet long in 200 coils—the outer, B, is 40·5 feet long in 200

coils or convolutions—wire silked. The thin wire galvanometer (15414) was connected with the outer coil of this compound helix and a battery of 5 pr. Grove's plates with the inner coil. Observing the needle directly by the eye—contact at the battery was seen to shake the needle by the induced current—then it settled in its first position; and then breaking contact—shook it again and then it settled as before. The action was very good. A single pair of Grove's plates could produce the same effect though not so strongly.

15416. Now connected the thick wire galvanometer (15413) with the helix and battery in like manner. Observing by the eye only, the same effect was produced by making and breaking contact but not so strongly as before. Even one pr. plates produced a sensible effect.

15417. Instead of the compound helices, employed 2 flat helices. They consist of wire about the same diameter—each helix is 5½ inches across and contains 53 feet of wire. One of these was connected with the thick wire galvanometer (15413)—the other with the voltaic battery of 5 pr. of plates. When the two helices were laid one on the other, the needle observed direct by the eye was moved at every occasion of making and breaking contact. When the helices were separated by books placed between—the space being 2½ inches—the needle was affected but only in a small degree. When books to the extent of 8 inches were interposed, the eye alone could not detect any sensible motion in the needle, but on using the reflected ray of a candle placed 4 feet off, the needle was seen to move, for the reflected image moved.

15418. Now replaced the thick wire Galvanometer by the one of thinner longer wire (15412)—the helices being as before; and observed by the reflected ray. Motion of the needle appeared when the helices were 14 inches apart—and even when the distance apart was made 22½ inches, the needle moved.

15419. When one helix was held in a plane *perpendicular* to that of the other helix and either near to it or about 4 inches off, there was motion in the galvanometer needle on making and breaking contact. Should that be the case if properly adjusted? I had not time to complete the trial.

15420. For the purpose of comparing the effect of a fine wire coil, took the needles out of my Ruhmkorf Galvanometer and introduced the needle used before (15407, 13)—and then associated the two flat inducing helices (15417) with the battery of 4 pr. of Grove's plates with it. This Galvanometer coil was not so good as the one of 43 feet of medium wire (15412). When the coils were six inches apart, the needle was scarcely sensibly moved on making contact and induction. Changed the needle to the coil of 43 feet and then the needle was equally affected when the induction coils were 16 inches apart. So must use this coil or a coil of such wire. I employed an Argand lamp as the source of light here; because of its size it subtended a larger angle and the image did not disappear so suddenly. The light should be small and intense.

15421. If a power, as magnetism, acting statically inversely as the square of the distance, could be proved to require time for the establishment of its action, i.e. time dependant on mere distance, what effect would that have upon the reasoning to such a case as Gravity?

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15422. I have constructed four induction spirals named A, B, C and D. A and B are concentric on one frame and may be conjoined together and moved to different distances together, but can not be moved in relation to each other. A is 80 inches diameter, B 34 inches diameter; each consists of three spirals; side connecting pieces are soldered on so that either 1, 2 or all 3 of the spirals may be in circuit at once.

15423. C and D are associated concentrically on another moveable frame, so as to be separable to any distance from the former. Each has two spirals. C is of thinner wire than D and is a circle 65 inches in diameter—D is a circle of 58 inches diameter.

15424. *Induction helices.*

	Thickness of wire	whole length	diameter of circle	No. of spirals
A.	0·175 of inch	62·83 feet	80 inches	3
B.	0·175 of inch	26·5 feet	34 inches	3
C.	0·1 of inch	34 feet	65 inches	2
D.	0·175 of inch	30·33 ,,	58 inches	2
E.	0·175 of inch	7·166 ,,	27 inches	1
F.	0·100 of inch	4·66 ,,	9 inches	2
F *a*.		15·17 ,,	29 inches	2

See (15503)

15425. Galvanometer employed—that before described (15412, 8) containing 43 feet of wire—needle a square prism, short as before, reflecting well. Used small lamp flame (15407) about 30 inches off and observed at the same distance—the motion of the needle was easily seen.

15426. In these trials, I use the lines of force resulting from the induction currents only, i.e. I exclude an Electro-magnet, because the time occupied by the iron core in taking up its state is considerable and I desire to exclude all consumers of time as much as possible from the proposed examination of the *time of distance*.

15427. In the following experiments the various helices stood North of the Galvanometer in a vertical position, their planes being east and west. The connexions were by tinned wire ends and cups of mercury—such perfect and capable connexion is requisite for the induced currents passing through the galvanometer.

15428. The helix A (15422, 4), with all its spirals in the circuit of a five celled Grove's battery, affected the Galvanometer at the distance of 14·5 feet pretty well, i.e. by its direct action and without any induction. The spiral B (15422, 4) with its 3 convolutions scarcely affected the galvanometer directly at the same distance.

15429. The helix C (15423, 4) at the distance of 7 feet, with both its convolutions in same battery circuit, affected the Galvanometer needle a little—it is of thin wire. The helix D (15423, 4), at the same distance, battery and other circumstances, affected the Galvanometer directly in a sensible degree and more than C did: it is a smaller coil, but then the wire is thicker and hence its advantage.

15430. So thickness of wire is good.

15431. Even with one spiral at this distance the Galvanometer was directly affected.

15432. Now experimented with induction currents. A, B are 14·5 feet from the Galvr. C, D are 7 feet from it; so A, B are 7·5 feet from C, D.

15433. First made C an inductric spiral with battery of five cells; and D, which is within C and only 3·5 inches from it, the inducteous circuit, connecting it with the Galvanometer. At every

making and breaking of contact with C, there was a very strong effect at the Galvanometer.

15434. B with its three convolutions the inductric circuit–D with one spiral the inducteous circuit. The spark at B was good–the needle affected on making and breaking contact. When D was made inducteous, with both its convolutions, then the needle much better affected. Here a distance of 7·5 feet between the two spirals. Also the value of two or more spirals in the inducteous coil proved.

15435. B one convolution inductric–D one convolution inducteous: action on the galvanometer needle but very weak. Then B one, inductric, and D 2 inducteous–the effect on the needle much improved. B two, inductric–and D two, inducteous: still further improved. B three, inductric, and D two, inducteous: the galvanometer effect is still more exalted.

15436. So adding to the inductric and also to the inducteous coils does good.

15437. Now made the largest helix, A, the inductric and the smaller thicker wire helix, D, the inducteous coil. A, one circle inductric–D, one circle inducteous, 7·5 feet apart: acts fairly on the galvanometer needle. A, two circles inductric, and D, one circle inducteous–acts well and better on the needle. A, 3 circles inductric, and D, 1 circle inducteous–acts very well and better than the last. A, 3 circles inductric, and D, 2 circles inducteous–best action on the needle and strong. Value of the additional circles both in the inductric and inducteous coils well seen here.

15438. Now made A, 3 spirals, inductric–and D, 2 spirals, inducteous, but removed the latter 13·5 feet from the former. The Galvanometer needle was well moved through this distance.

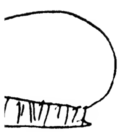
15439. Laid D, 2 spirals inducteous, horizontal in the middle of the experimental room. A, 3 spirals, was made inductric. There was action on the needle, but D was not put carefully in a plane in the axis of A and unless it were so there ought to be action. Must not trust to such an arrangement for removing a length of wire out of the inductric influence.

15440.

Exp. room.	Servants' room	Housekeeper's room	Jane's room	Servants' room
feet 14	12	20	14	14 feet

Measures of distance up stairs–whole length 74 feet.

15441. Worked with the inductric helices, etc. in Jane's room, i.e. 46 feet from the inducteous helices C, D; for C and D were connected together properly and used as one helix in communication with the Galvanometer (15425). The inductric helices were A and B, also connected properly together, but for convenience sake the battery of 10 pr. Grove's plates was in the experimental room, connected by thick wires 0·175 of inch diameter amounting to 180 feet together in length with the inductric helix, for in this way contact could be made and broken at the battery with readiness. Effects were obtained on the Galvanometer and at first referred to induction through the 46 feet; but on testing the results in various way[s], as by disconnecting the galvanometer from the inducteous helix, etc., when action on the needle was still found to occur, the whole nearly were referred to the current in the parts of the wire near the Voltaic battery; for the battery being raised, the wire to one end rested on a chair and that to the other end on the ground for some feet of distance, and this formed (with the battery) a loop in the circuit, which was the real and most effective inductric circuit; being only 6 feet from the inducteous circuit, it affected it well, and when that was disconnected, affected the needle directly—even when a thick block of iron $7 \times 7 \times \frac{7}{8}$ inches was before it—or another block $5 \times 2\frac{5}{8} \times 1\frac{1}{8}$ inches in size.



15442. To confirm this result, used a thick wire in form of a loop to connect the ends of the battery in the room. When the loop large as 5 feet in diameter—action of Galvr. strong—but when reduced to a circle not more than 18 inches diameter, which was less than the loop in the former connexions—effect on the needle was produced.

15443. Threw out this interfering effect by bringing the battery wires on to the same level, so as to annul the loop action; this being arranged, when A, B was inductric in Jane's room and C, D inducteous and connected with the Galvanometer—the effect of induction upon the helix and needle was sensible whenever contact was made and broken, but it was very small, 10 pr. of plates being used and a good breaking spark obtained. When the ends of the conductors in Jane's room were separated from the inductric helix and merely connected together, then making and

breaking contact did nothing, shewing that the helix inductive action was real.

15444. Now employed our largest cylinder Electro magnet as the inductric in Jane's room, it being 55 feet direct from the inducteous helix. The spiral of the Electro magnet contains 79 feet of thick wire with 4 feet of ends–its effect was not sensible.

15445. The iron core of this helix is a cylinder 1·75 inches in diameter and 21 inches in length. There was now a much increased breaking spark, shewing the influence of the core, but the effect on the helix C, D was not sensible, the 10 pr. of plates being employed.

15446. I wonder this Electro magnet does not produce a sensible effect as the helix A, B did. I should have thought the sphondyloid of statical or continuous power belonging to it would have been stronger as a whole than that of the helix A, B. Perhaps if it were so the time of its rising up may be very much longer (as many circumstances seem to shew), and then its effect in shaking the needle would of course be greatly diminished, as the impulse would be prolonged and feeble instead of sudden and powerful.

15447. *Galvanometer.* The lamp was about 18 inches from the needle and the eye receiving the reflected ray from 3 to 5 feet distance. A carriage passing by makes the needle tremble. An eye glass assists my sight–an opera glass, unless held fixedly, gives uncertainty because tremor makes the apparent place of the reflector needle vary. The reflecting face is very imperfect and gives a diffused image–the lamp flame is too large and too near, so that reflected image does not disappear at once.

15448. A screening block of iron does by itself displace the position of the reflected image more or less. May probably use a piece of soft iron as a means of adjusting the place of the image in the complete apparatus.

15449. Towards end of the Evening removed the Voltaic battery near to the door of Jane's room and then the effects were as just described (15443). In this case, no interfering loop action could occur.

15450. *Galvanometer* of *Becker.* Copper wire $\frac{1}{27}$ of inch in diameter–about 184 feet in length in about 500 convolutions–

1 foot weighs 2·12 grains—whole weight is 389·5 grains, so length 184 feet. Marked Galvr. C.

15451. The Galvanometer of 43 feet of wire 0·033 of inch diameter, containing about 120 convolutions (15412) is marked A.

15452. The Galvanometer of 38 inches of wire 0·175 of inch diameter in 8 convolutions (15412) is marked B.

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15453. Induction results as to length of connecting wires and also delicacy of Galvanometers (15450, 1, 2).

15454. The Inductric and inducteous spirals (15424) were placed only 3 feet 6 inches apart—but they were removed to a direct distance of $58\frac{1}{2}$ feet—so that the battery to the inductric spiral was away from the galvanometer; and the connecting wires between the Galvanometer and the inducteous spiral amounted to twice 80 or 160 feet—thick copper—connected by mercury joints. The Spirals were parallel to each other and stood with their planes north and south at the end of the attic passage. Trough 10 pr. Grove's.

15455. The Inductric coil was A, using its 3 spirals (15424). The Inducteous coil was C, D, four spirals. The Galvanometer was A (15451) or the best. 10 pr. of plates produced strong action on the needle on making and breaking contact, as was to be expected with so short an inducing distance as 3 feet 6 inches. Two pair of plates produced a strong action one pair of plates an excellent action. On disconnecting the galvanometer from its wire, then the 10 pair of plates produced no effect. So that thus the battery is removed from direction action on the needle—so is the needle from the direct action of the Inductric coil in this position and distance—and the induced current is shewn to run easily through this length of thick wire.

15456. Reduced the Inductric coil A to 1 spiral—the inducteous coil remaining as before. 10 pr. plates still produced an excellent effect—2 pr. were very good and even 1 pr. good.

15457. Kept the inductric coil at A 1 spiral, but reduced the inducteous coil to C, only of 2 coils. Still 10 pr. of plates gave an excellent effect—2 pair and also 1 pair good effects.

15458. Now the Inductric was a spiral or coil of thick copper

wire 0·18 thick—the helix as a whole contained 37 spirals or convolutions and was 2 inches in diameter—it was placed nearly in the axis of the Inducteous coils C, D; and with Galvanometer A (   ) produced fine effects with 10, 2 and 1 pair of plates. Reduced the inducteous to one spiral of D and even then there were good effects with 10 and 2 pair of plates—but with one pair the result was not sensible—I suspect because the contacts were not sudden or perfect for such a current at the Inductric circuit.

15459. Made the inductric 20 convolutions of the helix (15458). Good effect, but not so good as the whole. With 10 convolutions and 5 and also $2\frac{1}{2}$ there was effect, but now found that the battery connections formed a loop which, as before (15441), was all this time affecting the inducteous helix and which in the last condition of the inductric of 2 spirals and 1 spiral produced the whole effect. *The Inductric conductors must be parallel and close to each other* in all cases so as to avoid such results.

15460. Dismissing this small coil helix, I returned to B as the inductric, having 1 convolution of B and 2 convolutions of the inducteous C. 10 pr. and 2 pr. of plates affect the needle of Galvanometer A well, and 1 pr. does so in a very useful degree. Changed the Inducteous C, which is not the thickest wire, for inducteous D which is the thickest, and has 2 coils also, but somewhat smaller; the effects were about the same.

15461. Now compared the three Galvanometers (15450, 1, 2). When the inductric was B, 1 convolution, and the inducteous D, 2 convolutions, the Galvanometer A was well affected as just said (15460). When the thick wire Galvanometer B was used, 10 pair of plates did affect it, but poorly, and the instrument B seems far below A in sensibility. Now employed the Galvanometer C of far longer wire but obtained no sensible effect at all; it was far below A or even B (15464).

15462. In order to ascertain whether this was due to want of inducteous connexion, the Inductric was made the whole of A, B and the inducteous the whole of C, D. Now 10 pr. of plates produced a fair shake of the needle—but still the effect was very poor as compared to that of galvanometer B or A.

15463. This Galvanometer C has its needle suspended by a cocoon thread more stiff and rigid than that of the others—it by

torsion deflects the needle much from its true magnetic position: by a little magnet I constrained the needle into a convenient position, but all that would place the needle in a very rigid condition, so that in truth no fair comparison could be made between this needle and the very tremulous one of A or of B. Must provide for all this in the Galvanometers.

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15464. Examined the three Galvanometers (15450, 1, 2). Galvr. A, medium wire, bore 5 torsion rotations between the upsetting positions right and left, the needle being influenced by the earth's force only the needle was evidently very good. Galvr. B, thick wire, bore only $3\frac{1}{2}$ rotations. Galvr. C, long medium wire, scarcely sets at all and cannot bear one revolution—in fact the needle has not been magnetized except by some accident. So no wonder it gave no results when tried (15461). So both B and C were not in fair comparison for one cause or another.

15465. First selected a suspension film—drew this out from a skein of fine flos silk, where the fibres are very fine and where also they have been cleansed from Gum, etc. by washing and preparation in the manufacture. It is far finer than any mere cocoon silk that I have.

15466. Next tried different lengths of magnetic needle—a square steel needle (15405) hardened, etc., about $\frac{1}{4}$ of an inch long, was well magnetized. It was then broken into two pieces, one nearly twice as long as the other. The *short* piece was attached to the end of the new silk film (15465) by a short piece of bristle and soft cement, and was then examined whilst under the influence of the earth. It bore 28 torsion revolutions between the two upsetting points, shewing the goodness of the needle and also the flexibility and fine condition of the suspension film. When standing at liberty, i.e. nearly free from torsion force, and set vibrating, it gave 42 and 43 complete or double vibrations in $\frac{1}{4}$ of a minute, or 170 vibrations in one minute of time. This is a very good state of needle and suspension.

15467. The longer piece of needle (15466) was then attached to the same suspension in place of the short piece. It required as

many as 82 torsion revolutions between one upsetting point and the other—shewing how much stronger the directive pull is by such increased length. When set vibrating as before, it was slower than the former, giving only 140 complete vibrations per minute. This shews that its inertia is increased in a greater proportion than its directive force. The short piece will probably be more momentary in its indications of a passing current than the long piece.

15468. The needle of A was now re-examined (15464), being on its own first suspending fibre. It required 6 careful torsion revolutions between the upsetting points and it gave 126 complete vibrations per minute. This needle is square and is I suspect part of the same steel bar as the two pieces above (15466). Now attached it to the new suspending fibre (15465) and then 51 torsion revolutions (instead of 6) were required between the upsetting points—and about 170 double vibrations in a second of time.

15469. I now furnished all the Galvanometers A, B and C with new needles and new suspenders. The needles are all of Newman's square steel needle, which though convex on the sides and not well polished will do for the present—they are all short but not alike in length—C has the longest needle A the shortest B is intermediate.

15470. The suspensions are all of the fine white flos silk—but it cannot be otherwise than that they should differ in torsion force—though it is no doubt less in all of them than in ordinary cocoon silk.

15471. Galvr. A needle out of, i.e. on one side of, the coil but with the proper length of suspension, required $12\frac{1}{2}$ torsion revolutions between the upsetting points—it is the shortest needle and that agrees with the result. It gave 178 double vibrations in one minute, or at that rate.

15472. Galvr. B needle gave 12 torsion revolutions between the upsetting points—and 126 double vibrations in a minute, or at that rate.

15473. Galvr. C needle required 12 torsion revolutions between the upsetting points. It gave 168 double vibrations in a minute, or at that rate.

15474. To compare these Galvanometers as to their sensibility

of induced currents, I took 24 feet of copper wire $\frac{1}{34}$ of an inch in diameter, made a helix of two coils at about $\frac{1}{3}$ of its length from one end, connected the ends by mercury with each of the Galvanometers in succession and induced a current in the wire by passing one pole of my single Logeman horseshoe magnet through the two convolution helix. The motion of the magnet outside the helix produced no effect on any of the galvanometer[s]—a single passage of a pole in or out of the helix did affect them. A and C, being the thinner longer wires, were best affected. B or the thick short wire was very poor indeed—it of course carried all the current induced but wanted convolutions. I think the greater length of C wire did not do any good, for it was not better than A—the two were very much alike; probably a Galvanometer of intermediate length, of the same wire, would have been better than either for this induced current. The effects were equally good at making or breaking contact—either may probably be employed in the final arrangement.

15475. Repeated the trials, employing a thick wire $\frac{1}{10}$ of inch in diameter and 12 feet in length—having only one convolution instead of two—it was connected by mercury with the Galvanometers as before and the same inductric magnet employed in the same way. Now B or the thick wire galvanometer was the best, though having only 8 coils or convolutions; then came A and then C not far behind. If there had been two convolutions in this wire, it is very probable that both A and C would have beaten B. Must try all three with the large spirals up stairs.

15476. I shall want in the final Galvanometers a means of revolving the point of suspension about a vertical axis, so as to undo or arrange any effect of torsion and set the needles free from that.

15477. *Position.* The position of the inductric and inducteous helices to each other and to the Galvanometer needle is important. Considering the Sphondyloid of power, i.e. the lines of force of the inductric helix, the inducing influence must be greatest when the inducteous helix is parallel to and concentric with it, i.e. having the same axis whatever the distance may be—for if the inductric coil be turned 90°, then its lines of force will exert equal and opposite force on the parts of the inducteous coil which

are situated at the two opposite ends of any diameter, and so no induced coil [? current] can occur. Or if the inducteous coil be turned 90°, the same balance of induction force will occur. It is indeed evident that if the inducteous coil be at first parallel to the inductric—the greatest induced current in the former will occur; that if it be revolved 180° so as to be again parallel, a like strong current in the contrary direction through the coil will be induced, and that whilst passing through the half revolution it will at 90° be in the neutral or indifferent position.

15478. If the inductric helix be truly magnetic north or south of the Galvanometer needle, it will add to or take from the earth's power, but not tend to displace the needle. The same result would happen if the inductric helix was either E. or W. truly and magnetically of the galvanometer. So any of these four positions may be taken up—and then the Inductric and inducteous spirals placed in plane parallel to each other and on the same axis.

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15479. *Inductric*, A, B joined together, 6 spirals in one series, excited by 10 pair of Grove's plates and placed in Jane's room C, D joined together in 4 spirals placed in the experimental room, 33 feet from the inductric spirals and with 4 walls or partitions intervening. The three Galvanometer[s] were 14 feet further off and connected with the inducteous coils by either thick or thin wires and mercury connexions.

15480. The thick wire[s] were one 0·2 and the other 0·175 of inch in diameter and 17 feet long each. The thin wires were 0·055 of inch in diameter and 24 feet each in length—all of copper.

15481. When the galvanometers (15469) were unconnected with the inducteous circuit, the direct action of the inductric coil was scarcely sensible on them—perhaps it might be in a very slight degree on A and C, but doubtful.

15482. When the inducteous coil was connected by the thick wires with Galvanometer A (15471), the inductric coil exerted a very little action on it—it was hardly sensible; when the thin wires (15480) were employed as connexions there was the least possible action.

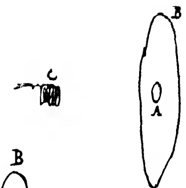
15483. When the Galvanometer B (15472) was used with the

thick wires, the action was uncertain, scarcely sensible. With the thin wires it was not sensible.

15484. When the Galvanometer C (15473) was employed with the thick wires or with the thin wires—there was the merest trace of action, if any.

15485. Now changed the inductric and employed our large cylinder Electro magnet, of which the iron core is 1·75 inches in diameter and 21 inches long and the wire 55 feet in length (15444, 15445), leaving it in the right position and same distance in Jane's room. There might be a little inducteous effect with Galvanometers A and C, but it would be safer to count it as nothing with all three, whether the thick or the thin wire inducteous connections were employed.

15486. That the Inductric connexions were perfect was shewn by the spark at the battery. To try the inducteous connexions the two spirals of D were passed between the poles of my single small Logeman horseshoe. All the Galvanometer[s] were well affected whether thick or thin connexions were used. A and C the best.

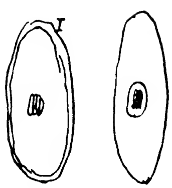


15487. So in fact the inductive power at the distance of 33 feet is too small with these spiral[s] and I must prepare better ones.

15488. *Considerations.* If A inductric be concentric with B inducteous in the same plane and also with C inducteous in a distant plane:—and if B and C contain the same length of the same wire, what will be the distances from A to B and from A to C, the effect at the galvanometer being rendered equal?

15489. Suppose A and B to contain the same length of wire; will there be any difference in *time* or *force* when A is inductric to B or B to A?

15490. When B is inductric, it will probably act much more powerfully on A than A on B—it will probably act also more powerfully on C (15488)—and so perhaps give the best arrangement.



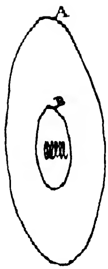
15491*. What of this arrangement? A being inductric to B and C inducteous. Ought the axis of the arrangement be parallel to the magnetic dip—or transverse—or will it be indifferent?

15492. Would either of these arrangements be important, the external or the central I being employed as the inductric?

* [15491]

15493. Experiments in the passage, 56 feet in a direct line from the galvanometers. The Galvanometer wires are $\frac{1}{5}$ of inch or nearly, and are each in one piece 75 feet in length. The Galvanometer C is as to its needle exceedingly tremulous—every little tap on the table and every carriage that passes causes vibration. The smallness of the weight of the needle is insufficient to stretch the silk fibre—it therefore has contortions in it, and vibrations up and down are by these contortions converted in part into a vibratory state of the needle. The instruments finally must be used in a very quiet place. The Galvanometer C has far more of this accidental vibration than A and B. I suspect this is because the supporting stem is screwed up tightly whilst the stems of the others are loose by comparison, and that this tightness transfers the house vibrations to the needle (15506).

15494. Ten pair of Grove's plates were employed in the following experiments.



15495. Provided three spirals—A of one convolution 21 feet in length and 80 inches in diameter—B of two convolutions 12 feet in length and 34 inches diameter—and S a helix of 23 convolutions and 12 feet in length, the spirals being two inches in diameter—all the wire was thick and alike 0·175 of inch—the spirals were concentric with each other and, except the helix in part, in the same plane.

15496. Made A inductric and B inducteous. With Galvr. A (15451) the effect was very good—with Galvr. C it was less—and with Galvr. B or thick wire very small. When a change was made to B spiral inductric and A inducteous, the effect was good as before but I think not better—the order of the Galvanometers being the same. So with this distance of 23 inches between A and B, whether the smaller or the larger were inductric or inducteous, the result appeared to be the same (15502).

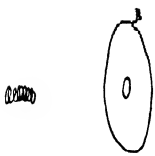
15497. Now worked with B and the helix S (15495), the distance between them being 15 or 16 inches and each having the like quantity of 12 feet of wire. B inductric and the helix inducteous—produced action on Galvr. A—and a little on Galvrs. C and B, but the result was nothing like so good as the former; it is true that A of 21 feet is changed for S of only 12 feet—but the interval is diminished. Instead of B inductric made A inductric, so now

the length of wire is increased again to 21 feet, but then the helix inducteous of 12 feet is 39 inches from it instead of 23 inches. The action was not increased, but nearly the same to appearance.

15498. The helix (15495, 7) was now made inductric and the large circle A inducteous—effect much as the last, not better. Also the helix being inductric the circle B was made inducteous, but still the effect was low and about as just before, or somewhat better perhaps. So no advantage presented by the wire in form of helix.

15499. In order to perceive the increased action of an iron core, the helix S (15495) was made inductric and B (15495) inducteous—the effect on making and breaking contact was marked. Then an iron core 12 inches long and 1½ inches in diameter was put into the helix and now its power as an inductric was *much increased*. Even Galvr. B shewed the effect well and Galvrs. A and C much more. That effect was clear.

15500. I then employed one single convolution of the thick wire, it being only 2½ inches in diameter and containing about 8 inches in length, and placed it concentric with B (15495): it was made inductric and B inducteous—the effect was only just sensible on Galvr. A. The helix of 12 feet (15495) was then placed in axis of small ring, the middle of the helix being 18 inches from the ring—but when the small ring was made inductric there was no sensible action. So the helix and the ring B were nearly equidistant from the small inductric ring—but the ring B seemed the best of the two as the inducteous arrangement.



15501. Now B was made inductric, the helix S being inducteous. When the latter was 18 inches from the plane of the former, there was scarcely a sensible action on Galvr. A—with a distance of 10 inches only there was a little action. With a distance of six inches only from the plane of B there was still very little action on the helix. The compressed helix does not appear to present a good form.

15502. Compared a large and smaller ring together: thus B one convolution of 6 feet length and 34 inches diameter and A one convolution of 21 feet in length and 80 inches diameter—wire alike and thick. Now when B was inductric and A inducteous, the action was good on Galvr. A. When A was inductric and

B inducteous, the action was not so good. When B was again made inductric and A inducteous, the effect was clearly best; thus the inner inductric and the outer inducteous appeared to be best (15496).

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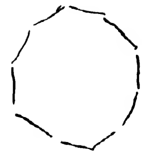
15503. Spirals, experimental, A, B, C, D, E, F (15424). G, a spiral on the old frame, replacing A for the time; it is of copper wire $\frac{1}{15}$ of inch in thickness, put on not quite in spirals but polygonally. There are 73 convolutions—they are one third of an inch apart the outer contains 19 f. 5 inches of wire (being inches in diameter) and the inner 6 f. 9 inches of wire (being inches in diameter). Consequently the whole length is about 955 feet. The 22 external spirals contain about 400 feet. The wire was in three pieces, so there are two solderings.



15504. H, a spiral on a new frame. Copper wire $\frac{1}{10}$ of an inch in thickness—48 convolutions half an inch apart, the outer 6 feet in diameter and the inner 2 feet diameter. The outer contains 18 f. 6 inches in length of wire and the inner 6 feet 6 inches the whole length of wire is 604 feet. There are three solderings. The nine outer coils contain 163 feet in length.

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15505. I, a flat helix of thick wire, contains 38 convolutions; the wire, copper, of the 28 outer convolutions is 0·175 of inch in diameter, that of the 10 inner convolutions is 0·215 of inch in diameter. The outer coil, which is 72 inches in diameter, contains 19 feet 2 in. of wire, and the inner, which is 34 inches in diameter, contains 9 feet of wire, so the 38 spirals contain 535 feet in length of wire.

15506. *Galvanometer* C. Needle always trembling—loosened the nuts of the stem and made the latter loose in the stand and then the vibrations complained off (15493) almost entirely gone. This is a good expedient to lessen these interfering vibrations in town or indeed any where else.

15507. *Inductric coil* A (15424), 63 feet of thick copper wire 0·175 of inch thick in three convolutions 80 inches in diameter—placed against North wall of Jane's room and connected by long thick wires with a 10 pr. plates battery in Anderson's sleeping room. Contact good—no direct action of any part of this arrangement on the galvanometer A (15451).

15508. *Inducteous coil* G (15503), 995[1] feet of wire $\frac{1}{15}$ of inch in diameter (thin) in 73 convolutions, making a flat spiral of 6 feet outer diameter and 2 feet inner diameter on a wooden frame. This stood against the north wall of the Exp. room parallel to and concentric with coil A and 46 feet distant from it. It was then connected by thick wires and cups of mercury with the galvanometer A, and this connexion was known to be good by interposing a feeble voltaic couple of Zinc, platina and pure water, the current of which passed freely.

15509. On making and breaking contact of the battery with the Inductric coil A, no sensible effect was produced in the Inducteous coil G and its galvanometer. Tried several times with the same negative result.

15510. Changed the *Inducteous spiral*, now employing H, a flat spiral on frame 6 feet outer diameter and 2 feet inner diameter and containing 604 feet of wire $\frac{1}{10}$ of inch in diameter in 48 convolutions. The connexions were good as before but here again the effect upon it of the inductric A was not sensible by the Galvanometer A. I employed the thick wire galvanometer B (15452), but there was not the slightest trace of effect.

15511. Changed the *Inducteous spiral* again, now employing I, a flat helix on a frame containing 535 feet of thick copper wire in 38 spirals—the outer of 72 inches diameter and the inner of 34 inches diameter—the 28 outer convolutions were of wire 0·175 of inch thick and the 10 inner convolutions of wire 0·215 of inch thick. There were three solderings, over-lapping and well made; the whole was on a frame and rather heavy. Using the *Inductric* A as before, there was no trace of action either on Galvanometer A or on the thick wire galvanometer B.

15512. So this *Inductric coil* will *not* do.

15513. Now used a more powerful *Inductric coil*, namely the

[1] Par. 15503 gives 955 feet.

large helix I (15505, 11) of thick wire—placed it in Jane's room in the place of the former and used mercury contacts for the ends of the connecting wires. Made flat helix G (15503, 8), of much small wire, the *inducteous* coil as before, at the given distance of 46 feet. On sending the current through the *Inductric* helix, there was a current in the *Inducteous* helix—and also on breaking contact—the effect was not much, but enough for observation at the galvanometer A.

15514. In order to see if any hiding or obstructing effect would be produced by the presence of another coil, I placed H before G, close to but not touching it, and sometimes left its ends open, or connected together as an endless spiral, but never connected with G—the latter having only its proper connexion with the Galvanometer A. When contact was made and broken, there was galvanometer effect—the effect seemed to be a little diminished when the ends of the coil H were open. When they were closed, I think there was a clear interference, but still effect was produced at the Galvanometer.

15515. Now leaving the thick wire coil I as the *Inductric*, I made H (15504), of wire of 0·1, the *inducteous* coil at the given distance of 46 feet—and using Galvanometer A, had a fair action about as before—quite enough for an observation. Tried Galvanometer B of thick wire with the same arrangement—there was a little action but scarcely sensible. Tried Galvanometer C (15450)—it was about as A—the indication was about equal to that of A but the needle is a bad reflector, being a rounded surface. Its action would seem better if the reflexion were good and distinct.

15516. Placed the two coils G and H before each other, connected them consistently into one coil of 1600 feet of wire associated as the Inducteous coil with Galvanometer A. There was a very good effect at making and breaking contact.

15517. I think the effect was perhaps more sharp and distinct at the *breaking* contact than at the *making*.

15518. On connecting the two coils G and H so as to oppose each other by their induced currents, there was scarcely any sensible effect left at the galvanometer.

15519. The *inductric helix* I (15505, 13) was removed further North and placed against the kitchen door, 66 feet from the

inducteous—it was also a little oblique, not being parallel to it. G and H were connected together (15516) as the inducteous coil and with Galvanometer A there was action at this distance but it was feeble. G alone as the inducteous coil produced scarcely a sensible effect. H alone as the inducteous was better than G, though its wire not so long. G and H together were the best.

15520. I think it very possible the intervening walls may affect the result.

15521. A single thick copper ring E (15424), 27 inches diameter and wire of 7·16 feet in length, was made *inductric*. Round it was a single thick copper ring D (15424), 58 inches in diameter, containing 15·17 feet of wire and of course at a distance of 15 inches from E. Another inducteous coil of 2 spirals, containing on the whole the same length, i.e. 15·17 feet of wire, was placed parallel to E in the prolongation of the axis. The object being to see, with equal action on the two inducteous circles, which was most distant, so as to take advantage of that disposition. It was not easy to distinguish the best action either by lowering the battery power or otherwise, but when at equal distances the effects seemed to me to be nearly alike at the Galvanometer.

1 JULY 1857.

Galvanometers (15450, 64–76).

15522. Mr. Becker has prepared for me a small square hard steel needle or prism, so well polished and so flat as to give by reflexion a true image of a distant object—it is about   of an inch square. It was well magnetized—then broken into small lengths, and these employed as needles in the Galvanometers A and C (15464–76). The suspensions are of fine white flos silk (15470) and a bristle end as before. The Galvr. A steel needle is about 0·0675 of an inch in length. That of Galvr. C is longer, being 0·115 of an inch in length. When the Galvr. A was on the table, a candle flame placed 6 feet off and the eye also 6 feet off, and when all was adjusted so that the incident and reflected ray made an angle of about 6° or 7°, the reflexion was very good and the reflected image looked at by an opera glass was very distinct and clear. When the opera glass was moved, the lines and curves of light were produced. The reflecting plane was not quite vertical but nearly so.

15523. The Galvanometer C with its longer needle, examined as to the reflective power of the steel, gave also an equally excellent reflected image. When the flame was 12 feet off and the eye 11 feet away, there was a feebler but good image—the reflecting plane was nearly vertical. The floor of my room is unsteady and therefore the needles and the reflected images were unsteady. There were draughts also and probably glass may be needed for protection from air currents.

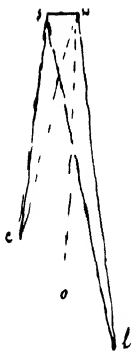
2 JULY 1857.

15524. Took the Galvanometers A and C on to the ground floor into the room called Servants Hall between Laboratory and Area. The room is on the earth and has a stone floor and in the N.W. corner of the room a square heavy block of stone is fixed on the floor by cement. Placed a large (green baize) block on this stone, with little wedges to keep it steady, and the Galvanometers on this block—found excellent results as to steadiness, etc. first.

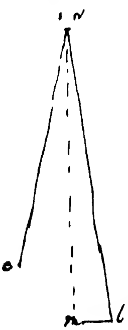
15525. Galvr. A (15522) on the block—a wax candle flame at 9 feet distance—and the eye placed at 6 and 9 and 12 feet. The reflected image was very steady whether the galvanometer was covered with its glass shade or not. A hand-blow on the top of the stone did not sensibly shake the reflected image; such a blow on the side of the stone did affect the needle—the image went out of sight, and then as the reflector vibrated, returned by swings to its rest position. A mere finger tap on the wooden box produces great disturbance. The passage of a magnetic penknife or of my spectacles distur[b]ed the place of the image when they were from 1 to 3 or 4 feet from the needle.

15526. The image reflected was good and bright but would require to be made brighter for use, because of other reflexions required—and the enfeeblement consequent upon the use of a moving mirror. So proceeded to examine what kind of light would be best for use. That from a single candle, as already said, was good. When the flame was 9 feet off and the eye 7 feet off—a ring (vertical) was adjusted to fix the place of the reflected ray, which passed through its center, giving there the most luminous image. The diameter of this ring, being $1\frac{3}{8}$ of inch, would at its distance subtend an angle of 56′, not quite 1 degree. Now when

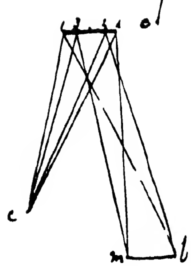
adjusted so that the brightest reflexion should be in the axis of the ring, the motion of the eye up or down or to one side or the other did not put the image out at once, but it dimind. in brightness and disappeared only when the eye ray was near to the edge of the ring, i.e. the eye might be moved across a space of nearly 50′ and yet see the reflected light more or less during the whole time. The light did not disappear suddenly and therefore if the eye were fixed, motion of the needle would not cause the reflected image instantly to disappear. But I want a quick and sharp disappearance, not one that comes on slowly or is gradual from light to darkness. Then as to the remedy.



15527. Let *e* be the eye, *l* the light as a mere point, and SN the needle reflector, all in plan. Then if the first position were such that the ray *l* S were reflected to the eye at *e*, the ray *l* N would be reflected to the place *o*, but if the mirror SN revolved or moved a little clock fashion, that ray *l* N *o* would come into the position of *l* S *e*, and until that moment the light *l* would be visible from *e* in some part of the reflector. Supposing SN to move with a constant velocity, this time would be longer with a longer mirror, and indeed sensibly as the length. *So the reflecting needle should be as short as possible to give a quick disappearance of the image on moving.*



15528. Now assume the mirror to be very short and the light a point: the disappearance will be instantaneous. But make the light broad, as *lm*: then if the mirror reflect the ray *l* SN to the eye in one position, it must move clock fashion before the ray *m* SN will be reflected to the eye. All the time of that motion will present an impression of the light *ml* to the eye, *and that time will be longer as ml is broader*, and in sensible proportion to the breadth.



15529. A large light, besides prolonging the time, must come in and go out gradually. If the reflector revolved clock fashion slowly, that point of the light *ml* represented by *l* will first come into sight in the reflector at 1; by the time that has passed on towds. 3, more of the light *ml* will be seen in the reflector; when *l* is reflected at 2, *m* will be reflected at 3 and all the light will be in the mirror—as the motion proceeds the image of the light will cross the mirror, going out at 4, and will disappear when

the last ray from *m* is reflected to the eye from 4. *So the light ought to be as narrow as possible to cause its sudden appearance and disappearance in the mirror.*

15530. Now for some experiments. Employed a lens 4 inches diameter before the candle, sending the image of the candle on to the reflecting needle. The reflected image was much brightened by this, but then it was much extended and the eye had to travel considerably to the right and left to lose the light, and still more up and down because of the elongation of the flame upwards. This accords with (15529), for the lens virtually presents a large luminous object. It shews that to obtain more light, the size of the light should not be increased, but the *intensity*. A very bright point is the thing wanted, or a very narrow line.

15531. To obtain a line of light, a copper plate had a notch cut in it $\frac{1}{16}$ of an inch in diameter, and this being placed before the candle so as to limit its apparent size to that dimension, the reflected image was regarded -the light was very poor and insufficient.

15532. A glass tube was used as a chimney to draw the candle flame up into a thin narrow column. There was no improvement in the reflected image and the candle soon burnt very badly. This arrangement might be made with gas jet and even oxygen supplied to the flame to increase its intensity.

15533. I ignited a vertical platina wire, by 3 and 4 pr. of Grove's plates, just short of the fuzing point—the light was very bright and was used as that to be reflected. But the reflected ray seemed as broad, i.e. as diffused, as with the candle unshaded, and the light not so good. Still, as this light was very narrow, it would seem as if the cause of the diffusion must be in the reflector. I employed the lens with this light--the reflected light was then bright but the size or aberration large, as it ought to be. I removed the lens and also the French shade which had up to this time covered the reflecting needle, but still the image was diffuse and did not come in and go out at once.

15534. The aberration must I think have been due to the needle—it was either too long or else was not perfectly smooth and true in the reflexion. The galvanometer A has the shorter needle of the two.

15535. Employed the Galvanometer C (15522). Using the open candle flame, the aberration was wider than before. The eye could see the image reflected when any where within the ring (15526). The needle of C was very steady when the Galvanometer stood on the box on the stone base—knocks on the stone or box acted just as before (15525). The use of the opera glass gave a good compact image—by moving the opera glass this image described lines or curves, very distinct if the motion were moderate—faint when it was very quick.

15536. When the box was tapped and the opera glass moved, the luminous lines were broken up into luminous and dark parts—but the point of separation or distinction was not very marked.

15537. Employed a moving silvered mirror in place of the opera glass—the effects were the same but the light very feeble if mirror motion was quick—tapping the support gave the light and dark parts in the lines.

15538. Employed the ignited platinum wire as a light—the effect was as before.

15539. 600 feet copper wire of 0·18 inch diameter = 64 lbs. nearly.
600 " " 0·1 " " = 20 lbs. "
600 " " 0·068 or $\frac{1}{15}$ " = 7 lb. 11 oz. "

15540. The *reflecting* needle. If it were rendered concave, that would compensate for the effect of length (15527), i.e. if the light were in the focus of the concavity so as to send parallel rays to the eye. Then the light might be much nearer also. A light, as the ignited platinum wire (15533), being 12 inches from the needle, would require a convexity having a radius of 24 inches—or a light 24 inches off would do with a convexity radius of 48 inches.

15541. The reflecting surface should be worked along its length, that the aberrations or prolongation of image (15534) may be across it or in a vertical plane and not in the horizontal plane. I have ordered such a concave needle of Mr Becker.

31 JULY 1857.

15542. K a helix on wooden frame, like H (15503[1]), of copper wire 0·1 of an inch in diameter. It contains 620·6 feet of wire in length in 49 convolutions half an inch apart, the outer of which

[1] Should be 15504.

contains 226 inches, being nearly 6 feet diameter, and the inner 78 inches, being nearly 2 feet diameter. It is provided with soldered ends and mercury cups, as the former are.

15543. So the thick wire helix I (15505, 13) is to be the inductric helix and the helices H and K combined the distant inducteous helices.

5 AUGUST 1857.

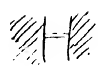
15544. Mounted a platinum wire, by soldering between two copper wires, for a constant light. The platinum wire was 1 inch long, in the vertical position, and $\frac{1}{120}$ of an inch thick. The electric current of 2 pr. Grove's plates was sent through it—but this was too much and it required 34 inches of copper wire $\frac{1}{65}$ of an inch thick introduced into the circuit to make the heat bearable by the wire (15533).

15545. Have prepared a moving reflector on an axis—the reflector is of thickish glass, silver[ed]—found on regarding a fixed light, as the ignited wire, that the double surface is bad—giving a blurred image. The reflector ultimately must have only one surface. *Steel* or *reflecting metal.*

15546. Placed the ignited wire light (15544) horizontally 16 feet from the moving reflector (15545), its axis being horizontal also—the fixed image being pretty well seen in day light and amongst the luminous things on the table—but when the mirror was moved quickly, the trace described by the light was very feeble to the eye and scarcely distinguishable, the motion being given by the hand only.

15547. Now the wire was put into a darker place, the eye being still in the same plane and at the same distance of 16 feet. The effect now much better and the image on the eye more sensible, and freely sensible with the quickest motion the hand could give to the mirror. The needle should be in darkness—the objects near it black—the eye also in darkness—and even the light of the wire carefully shaded.

15548. Now screened of[f] the wire by a card so that only a part about the $\frac{1}{5}$ of an inch was visible instead of the whole length of an inch. This was pretty well visible as a trace in the moving mirror. Then shaded off all but a part about the $\frac{1}{25}$ of an inch—this



light was feeble even to the naked eye when unmoving. When the reflector was moving, the trace could scarcely be perceived—but as yet the imperfect darkness was really a dull light and the eye was in day light and badly circumstanced.

15549. Placed the platinum wire vertical placed a flat galvanometer needle (15522) on the stone (15524) in the lower dark room in dull light and observed from a point near the light. The distance of the reflector from the light was 6·5 feet, and it was the same distance from the eye. Now the platinum wire was ignited, but its image reflected in the needle reflector was hardly sensible to the unaided eye—*not using* the moving reflector (   ) but the eye only; so that I do not think the platinum wire light will do, especially at such a distance as this.

15550. Placed a candle light in the place of the platinum wire ignited. It gave a moderately good direct image to the eye, but when the reflector (15545), now moving on a vertical axis, was put in place, the image was but feeble in that. When the mirror was moved, the line of light described to the eye was scarcely or not at all sensible. We want either the *lime light* or the *Electric light*.

15551. Observations on constancy of needle place. When brought down from attic into this underground room the needle set very slowly round, so that the reflected image is not thrown constantly to the same place—and this is probably due to the hygrometric effect of the air on the silk. The change at first is progressive. After a certain time the reflected ray place was noted and then all left for 1½ hours uncovered by the glass shade—there was a little change, and breathing with the mouth did not increase it. Being left open half an hour, there was no further change—this was at 4 o'clk. P.M.; being then left open all night until the next day at 10 o'clk., there was a little change (direct as before); this was marked and again all was left until   o'clk.

15552. When the glass shade is left on, there are unpleasant reflections of the light from it—a square or flat plate would prevent this, but it would be better open as the light must be weak and wants helping. Still, when open the currents of air affect the needle place and defence may be wanted for that.

15553. *Places.* Either the dark Exp. Room for the Galvano-

meters and the Laboratory for the helices, i.e. the laboratory and yard, etc. Or perhaps, the distilled water cellar and one of the others for the Galvanometers, and the laby., etc. as before for the helices. Or perhaps, the Glass house for the Galvanometers and the Laby., etc. for the helices.

15554. Now try lime light in the focus of the reflector concave needle.

15555. *Electrometers*[1]. The two will differ from each other but we can easily eliminate the effect of their differences by having a commutator so as to be able to alternate them one for the other—and then use the mean result.

15556. *Telescope.* I think I must use a telescope (if not an opera glass), not merely for defining the image and making it clear but also for *fixing* the *place of the eye.*

8 AUG. 1857.

15557. Took the Galvr. B (15452, 60) of short thick wire with its flat polished needle and experimented with it simply as a needle instrument, without the coils. The needle, being left to vibrate, made 20 or 22 complete vibrations (to and fro) in 10 seconds by the magnetic force of the earth. Then replaced this needle by another piece more recently prepared by Mr Becker (15559), having a *concavity*, part of a horizontal circle—this needle, before being broken off from the larger piece, gave a focus of about 20 or 21 inches with the parallel rays of the sun. The piece, when attached to the silk suspender in place of the former and vibrated, gave 22 complete vibrations in ten seconds under the earth's force.

15558. Now this piece is to sight nearly if not quite twice as thick as the first piece (15557) in both direction[s] and half as long again; and it must be about six times heavier; yet under the earth's influence is moved as quickly as the former. Therefore there is reason to hope any force in a coil round it would move it as soon as the former, and it presents a much larger reflecting surface, indeed three times as large: this will be an advantage if it hold good.

[1] "Gal." has been written before this word, as though it was intended to alter it to Galvanometers.

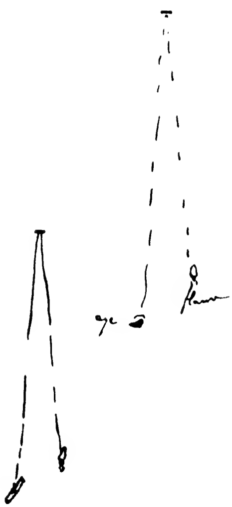
15559. Mr Becker has been preparing me some square hard steel needles of which one face only is polished—this face is made concave and it has been wrought in each case by working the tool chiefly along the face, so that when suspended with this face vertical, the lines if any will be in a horizontal direction and any dispersion they may cause will be in a vertical plane and not in a horizontal plane. They are to form needle mirrors concave in the horizontal plane, the luminous object being in the focus. Being examined in the Sun's rays, the foci seemed to be as follows, i.e. the image of the sun seemed to be most small and true at the distances set down.

15560. No. 1 . . . 14½ inches
2 . . . 12 inches—but 17 inches seemed to give equal effect of disappearance of the light over all parts of the image. When a reflector was too near or too far, then a card carried into the incident beams on one side cut off the reflected light on one side of the image first, either the near or the far side as the reflector was within or without the focus.

15561. No. 3—18 inches, the single image—increased up to distance of thirty inches, it gave two distinct images of the sun ○○, as if the needle had been bent in one place.

No. 4 . . . 20½ inches focus. A, B, C, D, E (15601).
No. 5 . . . 21 inches focus.
No. 6 . . . 27 inches focus—all about these numbers.

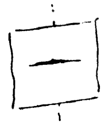
These all, as they ought to do, give dispersion in a plane transverse to the plane of convexity.



15562. Hung up this larger concave reflector (15557, 8) as before (15524) on the stone—placed a wax candle flame 6½ feet off and also the eye at an equal or greater distance. The light as seen by the eye direct in the needle reflector was very good—much finer than before—reflector needle and light steady.

15562½. Now replaced the eye by the moving reflector turning on a vertical axis. The light image was very good in this reflector. Being made most distinct for the middle of the immovable reflector, it went out as the eye was carried up or down or to the right or left. This shews the diffusion of the image, and that is a disadvantage to be avoided. But then the object or candle is large (15529) and it is also out of focus.

15563. When the eye was fixed and the reflector moved on its vertical axis, the trace of light was produced and pretty bright and visible with any velocity I could give. Of course the trace was brightest at the middle and fainter at the ends, and it did not go across the reflector, but appeared thus. Its appearance any where but at the middle was an effect of diffusion from the size of the candle and other circumstances.

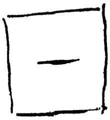


15564. Placed the opera glass for the eye (15562), observing the light in the needle directly through it. Seemed best to arrange the glass to give a good image of the needle, for then there was the least scattered rays and red light to the eye. The light was very good and not much *diffusion*.

15565. Increased the candle distance to $20\frac{1}{2}$ inches. The opera glass then shewed a good light. This is the focal distance for parallel rays. I could then see that there was a sudden appearance and disappearance of the chief light from the face of the mirror needle, but there was a good deal of irregular extraneous light from the ends and edges of the needle, appearing at the wrong time and continuing the general appearance of a light, gradually fading as the needle moved. Must touch these parts with black and perhaps also the upper and lower edges of the visible face; that will much improve the needle.

15566. Now as to *diffusion* of this concave unblackened needle reflector using a large candle flame. The candle was $20\frac{1}{2}$ inches from the mirror—it gave a diffusion to the eye $6\frac{1}{2}$ feet off in a horizontal direction equal to 1·5 inches from *out to out*, being brightest of course in the middle. With the candle at the distance of 34 inches the diffusion was less, perhaps 1·25 inches. Candle at 48 inches gave still less diffusion. Candle 6 feet off—diffusion less still. At $8\frac{1}{2}$ feet candle distance—diffusion was still less, being about 0·75 of an inch. This difference is evidently due to the smaller angle which the more distant flame subtends—but no doubt a part of the difference is due to edges and ends of the reflecting face. As to the *brightness* of the reflected image, that is greater, proportionately greater, as the candle flame is more near to the reflecting needle.

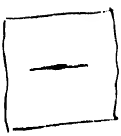
15567. *Disappearance of the light* by motion of the needle reflecting. The light a candle flame about 36 inches from the needle.



The reflector ($15562\frac{1}{2}$) *fixed* at the eye place and the eye stationary. Then the image, whilst still, appeared in the middle of the reflector at its best. When the needle was vibrated, the image described a trace across the middle of the reflector, the light going out by a longish sweep–and coming in in the same manner–that was as might be expected. This trace was longer as to the mirror as the candle flame was nearer to the needle reflector.

15568. I want a much narrower light than a candle, one like a star, or that subtends a small horizontal angle. To make a quick disappearance, it ought to be *distant*, *small* and *very bright*.

15569. *Platina wire light*. Proceeded to try this narrow light with the large reflecting surface. Used the platinum wire (15544) of yesterday, placed $6\frac{1}{2}$ feet from the needle in the first instance, the eye being about 8 feet off. It gave a good luminous image to the eye, very different to yesterday's[1] (15546) but not enough for the final purpose.



15570. At this distance of light the *diffusion* was more than I wanted or desired. As the eye moved right and left, the luminous object seemed to come in at one side and go out at the other; the light not being focal. When made to appear with the maximum effect in the middle of the eye reflector ($15562\frac{1}{2}$) and the eye kept fixed–motion of the reflector shewed the light and its diffusion as a graduated line. But I want a short mark, i.e. a bright light and at one spot only. The edges of the needle reflector are not yet blackened.

15571. When the needle was made to vibrate so as to give a disappearing or rather intermitting light, and the eye mirror also moved, it was not easy to distinguish the places of the maximum and minimum light in the latter.

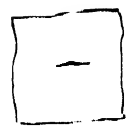
15572. When the eye mirror was away and the eye alone there regarding the light in the needle reflector directly–the motion of the eye to the right or left seemed to cause motion of the luminous image across the needle mirror, and that in the same direction as the eye, the wire light being at the distance of $6\frac{1}{2}$ feet. Part of this must have been due to the edges or ends of the needle reflector (15565).

15573. When the *luminous wire* was placed at the very short

[1] August 5.

distance of 16 inches only from the needle reflector, there was a good luminous image in the needle; far beyond that of yesterday[1] (15546). When the eye was moved right or left, the light in the needle reflector still seemed to move across in the *same* direction. When the eye reflector was in place and moved, the luminous trace was good, and I think that the images appeared and disappeared to the eye short and quick. It is probable that a platinum light will do.

15574. The platinum wire light (15569) 20½ inches or focal distance from the needle reflector gave a good star of light to the eye at its place. When the eye was moved to the right or left, the light in the needle reflector seemed to move in the same direction. When the light was looked at in the eye reflector, it was good, and on moving the reflector on its axis gave a short trace. Also when the needle reflector was set vibrating, I could on moving the eye reflector distinguish the maximum and minimum places, according to the place of the image. Still, the image is far from definite. As the eye was carried back from the revolving eye mirror, then the trace in the latter became better, i.e. more contracted in length and local.



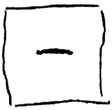
15575. The little voltaic battery fell at once in power, i.e. went down visibly judging by the wire ignition. On taking it down, found the S.A. in one cell quite neutralized to the taste. Recharged the two cells and found the ignition just as before (   ).

15576. Placed the ignited wire (15569) 6½ feet from the needle reflector. A good light to the eye in its usual place 8 feet off. As the eye was moved right or left, the light seems to redden first and then go out, first at the side towards which the eye moves, which is the reverse appearance to that when light is near the needle (15573). With the opera glass the light was good and clear—when moved right or left, red or obscure light comes in first at the side moved towards, so that the light *seems* to go out generally at the opposite side.

15577. Placed the ignited wire at the focal distance of 20½ inches from the needle reflector. When either the eye alone or with the opera glass was moved to the right or left, the effects were the

[1] August 5.

same as those just described. The going out results are probably due to the imperfect edges of the reflector (15565).



15578. But at this distance of the wire, the reflected maximum light was very good and bright, and is as sudden in disappearance as any of the former arrangements—perhaps more sudden. The trace in the moving eye mirror is not long—and if the needle mirror is vibrating, the maxima and minima of light are pretty well separated.

15579. When the needle was still, examined the diffusion of light in the vertical and horizontal planes, so as to verify theory: that in the vertical plane was very extended, as to be expected, both from the length of the wire vertically and the form of the reflecting surface—the horizontal diffusion is very small. When at the finest heat, the *platinum wire light* in the mirror is really good and may be very useful. If I use platinum lights, then better to make the two divide from one current—or perhaps better to make the two different parts of one current.

15580. The glass shade covering the galvanometer needle is bad in that it reflects points and lines of light from the platinum light and these fidget the eye. It should be away and all should be black about the Galvanometer and also behind it and beyond—as also behind the eye mirror; the eye should have nothing to observe but the trace of the moving light. Whilst the shade was on, one could compare a light from it and that from the quiescent needle reflector together in the moving eye reflector, as if they were two needle lights. I think I shall easily see whether two lights, placed by reflexion one over the other in the still mirror, appear one to the side of the other in the moving mirror, and that is what I want. Perhaps it may rise to the extent of making one of the images *disappear* before the other.

15581. Theoretically, the eye mirror might be curved, and then the eye being placed in the right point, the disappearance of one image before the other might be greatly facilitated.

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15582. I have *blackened* the extreme ends of the reflecting face of the needle reflector with Brunswick black—(Indian ink and water pigment will not do). I have blackened also all the bright

parts of the Galvanometer support with a dead water black—and placed a dead black cardboard behind the needle reflector at its place. It is a great improvement.

15583. Employed the platinum wire light (15544) at 24 inches from the needle reflector and the eye 8 feet off as before. The naked eye gives a bright light moving across the needle reflector in the same direction as the eye moves.

15584. With the *opera glass* at the eye place, adjusted to distinct view of the galvanometer parts and of the needle reflector itself, the light then appears as the smallest or most compact and very bright. As the eye and glass move to the right or left, the light comes in and goes out, preceeded and followed by a dull and rather long light. As the eye and glass move one way, the bright light appears to move the other. The light now has no elongation vertically but is as a star.

15585. When the opera glass was shortened so as to give distinct vision of objects 24 feet off, and so used—then the light in the needle reflector contracted horizontally and expanded vertically, so as to stretch across the image of the needle reflector as a vertical line (due to the position of the ignited wire), and this line moves to the right and left of the reflector as it slightly vibrates. When all was still, it was a still vertical column of light. As the eye and opera glass moved to the right, it moved to the right in the reflector, and as the eye went to the left it moved to the left also. The imperfections of the mirror are shewn upon it and also those of the eye, making the line irregular in its edges.

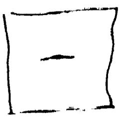
15586. Now lengthening the opera glass so as to make it fit for nearer and more near objects—the vertical line of light gradually shortened and the image of the mirror became smaller and full of light, and then the diffusion of the light was least, the light appearing and disappearing by motion of the eye very quickly. Being still more lengthened, the luminous image again became a vertical line across the reflector, and as the eye is moved to the right, it moves to the *left*, and vice versa. All this is right except that the needle mirror is not perfectly worked, for the reddening of the light as it comes in and goes out still continues when the opera glass is at the full light state of the needle reflector.

15587. There is much light from the general surface of the needle

reflector when the light (chief) is out, and this is not the reflected light of the room or a door way. I suspect it is the light from the platinum wire support of deal.

15588. *Blackened the wire support* in all parts toward the ignited wire and illuminated by it, and now restored it to its place, i.e. to 20½ inches from the needle reflector. Now the light in the reflector was much better, seen either by the eye alone or with the opera glass, and well *sudden*. Still, when the light *has passed* in the reflector, there remains straggling light, and by the opera glass this appears to be from fine lines or imperfections in the working of the surface.

15589. This distance of 20½ inches is perhaps the best.



15590. Put up the Eye reflector (15562½) there was an excellent light in it. When it was moved, the trace in it. Will do I think. Using the opera glass also in looking into the reflector, the effect was very good far better than before. When the reflector was moved to and fro, it gave a good distinct short trace.

15591. Thinking that the light emanating from the unblackened wire stand (15587) had caused much of the former effect, I cleaned off all the black from the needle reflector and then repeated the observations with the platinum light 20½ inches off. The effect to the eye was excellent either without or with the opera glass. But I could see light from the two ends when the chief light was out, and also by the opera glass, that they reflect a little light to the eye, so that a little blacking should be applied there.

15592. I think the *platina lights* will do.

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15593. Suspended the small flat needle reflector (15557) so as to compare it with this concave needle reflector (15557, 61), which is larger and probably six time[s] heavier, both in respect of obedience to the force of the earth or any other magnetic source and also as to the quickness of its disturbance; if its magnetic force is in anything like a proportion to the weight, there may not be much difference, and the large needle may be as delicate as the smaller one or nearly so. As a reflector it is very much better.

15594. Of these two needles the heaviest vibrates the slowest, but there is not a great difference. The number of vibrations in

a minute seemed to be about 128 : 140. Then swung them by the side of each other so as to ascertain their coincidences in direction; these seemed to be at 11 : 12, or again as 12 : 13, or again as 15 : 16. If these numbers express the earth's influence upon the two needles, they ought also to express the influence of equal electro magnetic currents. Probably the difference may be admitted and so advantage be taken of the size.

15595. When the needles were about $1\frac{1}{2}$ or 2 inches apart east and west, I tried to affect them equally by the quick passage of a distant magnetic bar: the smaller one then seemed most rapidly affected.

15596. I put both up at once in the Galvanometer place in the dark, each supplied with a wax candle flame as its object, at equal distances, so as to send the reflected rays to one eye place and therefore observable to one eye (or both eyes) at the same time. The light from the larger and concave needle reflector was vastly greater than from the other. It would *do*—the other was so feeble that it would not do in practise.

15597. Disturbed both needles at once by reversing a distant bar magnet 8 feet off. I could not see that the smaller one gave visibly the quicker indication—it might be, but the light was so feeble I could not be sure of the first moment of change. The other swung well, appearing to the eye to come in and go out very clearly.

15598. *Eye reflectors.* I have mounted a polished flat silver plate and also a polished steel button disk on the axis by the side of the silvered glass plate (15545), so as to compare these three reflecting surfaces as to their final use. The silver is pretty true in form and work, but may be more highly polished. The steel is well polished but is irregular, not perfectly flat and therefore giving a distorted image, the deformity being of course more as the eye receeds from the mirror.

15599. Used the curved needle reflector A (15557, 61, 601), a candle flame 5 feet off and the eye reflector 8 feet off. The image in the glass reflector was very good—also that in the silver reflector good—but that in the steel deformed because of its irregularity, else it would do too. As it is, probably the glass reflector silvered will do well for the first trials—using however a thinner

glass and allowing the incident light to approach more nearly to perpendicularity.

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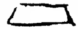
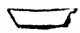
15600. *Galvanometers* A *and* C, being the two intended for the experiments. Blackened the bright stems and screws and other light or reflecting parts, making them of a dead black.

15601. Broke up the rest of the Becker concave bar (15561, 57) of $20\frac{1}{2}$ inches focus into four pieces. Ground the ends of these on a hone with oil so as to give them this form , the large surface left being the polished reflecting surface. Took down the former piece (15557) and ground it also. All are magnetic because the bar was magnetised before its first fracture. The former piece (15557) is now made A and the others B, C, D and E, being laid upon a diagonal scale and measured as to length (focus $20\frac{1}{2}$ inches):

A	was	45	parts
B	"	45·5	"
C	"	47	"
D	"	51	"
E	"	38	"

Each part was the $\frac{1}{400}$dth of an inch.

A was not quite perfect in the wedge shape at  one end; the other[s] were, except E, which was unground and irregular at the ends.

15602. The films of silk belonging to the Galvanometers A and C (15464, 522) were very like in thickness to the eye. The steel needle reflector A (15601) was attached to the Galvanometer A, being the one with smallest quantity of wire – the reflecting surface was towards the east and the reflexion was nearly in a horizontal plane.

15603. The steel needle reflector B (15601) was attached to the film of Galvanometer C (15464, 522); the reflecting surface was towards the east and the reflexion very nearly in a horizontal plane.

15604. When these needle reflectors were vibrated, the A reflector gave 108 double vibrations in a minute and the B reflector 110 vibrations (double) in the same time. So that they are nearly alike in this respect. I think these galvanometers are now ready.

15605. Brought Galvr. A with its needle reflector A (15602) into its place and position in the Experimental room. Arranged the platinum wire light (15544) at the distance of 6 feet from it and shall ignite the platinum by 2 pr. of Grove's plates. Arranged an oxyhydrogen lime light at the same distance, about 3 inches aside of the former, so as to have the two reflected images in the same horizontal plane and about three inches apart. Arranged the eye places about 8 feet off as before. The lime light was a cut corner of one of the cylinder[s] of lime—the oxyhydrogen flame was not rendered exceedingly strong but comparitively moderate.

15606. Being compared either by the naked eye or in the eye-reflector or otherwise, the lime light far surpassed the platinum light in its intensity—it was a beautiful sunny star as seen in the eye reflector and gave a fine mark in it when moving.

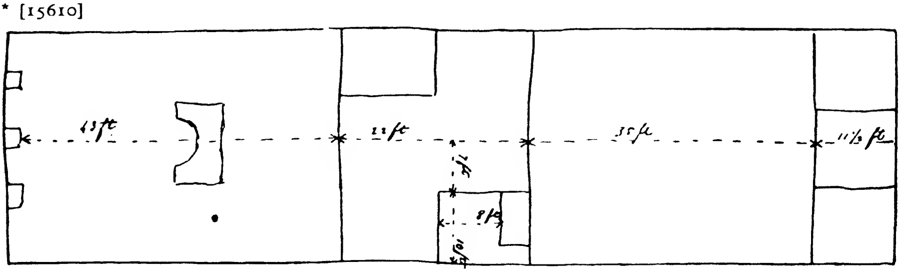
15607. As to diffusion of the lights that of the lime was greater in the horizontal direction than that of the platinum light, but not so much as I expected; the dull light before and after the chief light was prolonged, but I had not blackened the different parts of the apparatus that would light up by the lime. Must do so. The lime of course presents a wider source of light than the platinum, but not so wide an object as a candle.

15608. Using an edge of chalk, it gave a more compressed and an excellent light—it was a very fine light in the needle reflector and also in the Eye mirror.

15609. The lime light will probably be always more diffuse than the platinum light—because the oxyhydrogen flame is rendered luminous considerably by the lime particles: but I think the light may be very valuable, especially if the observations can be made upon the motion of the eye mirror putting one of the two lights out before the other. Must have both platinum and lime lights, and when the observation has been made with the former then put on the latter and repeat or confirm it.

15610*. Measures along the laboratory, the yard and the basement floor. From the South wall projections to the North wall of the laboratory is 43 feet—from that point across to the north face of the yard wall is 22 feet, and from that to the north wall or

* [15610]

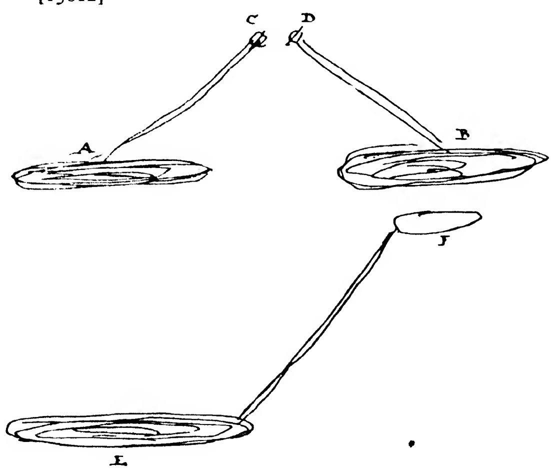


partition of the lower apparatus room is 35 feet, and from that to the North end of the distilled water cellar is $11\frac{1}{2}$ feet, making altogether about 112 feet in length. The glass house in the yard is 8 feet from N. to S. between the shelves and the heating furnace and 10 feet from E. to W. between the wall and the door -7 feet from the door reaches to about the middle of the open yard.

15611. Have ordered an additional lime light from Mr Becker. Have also ordered an eye reflector axis from Mr Newman. Have also ordered three plano cylindrical lenses from (New road), an inch square each and silvered on the convex sides, to serve as concave eye reflectors.

15612. In respect of the arrangement of the two inducing systems that have to be compared, probably the two inducteous helices ought to be alike in length, character, etc., that retardation from length of wire, induction amongst the spirals, etc. may be proportionately alike in both. But the inductrics acting on the two must be of different strengths so as to produce something like equal currents in the two. Still, the inductrics must both be excited by *one* current, that the identity of time at the beginning of the induction may be CERTAIN. Theoretically this arrangement* would do. A, B, two equal inducteous coils with their galvanometers C, D. Then E, a large inductric coil and F, a small one, connected together in one circuit with the battery, F being of such power as to produce an induced current in B of the force corresponding to that in A. In order to render the systems AE and BF independant of each other, they may be arranged in planes vertical to each other; thus A and E may stand in vertical

* [15612]



planes at each end of 100 feet, inducing horizontally*, and BF in horizontal planes, or in vertical planes at right angles to the former, adjusting the positions by trials so that the inductric E should have no action on B, and the inductric F no action on A; also that the inductrics E and F should have no direct action on the Galvanometers. Perhaps that may require the common axis of E and A to be horizontal and also magnetic north and south; in which case the joint axis of F and B could hardly be horizontal but might be made vertical, and so as to render the needles independant of F.

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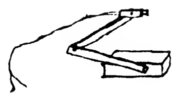
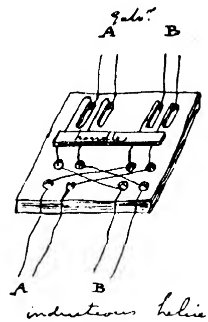
15613. *Observing Telescope.* Gives good image of the needle reflector at 8 feet distance. Shews it long, horizontal ⊂⊃ and full of light when a candle is 6 feet distance. As the reflected light always appears in one place, it is evident the eye reflector may revolve either in the vertical or horizontal or any other plane when looking for the disappearance of the light. Only when the two reflected lights are to be observed at once, the axis of revolution must be parallel to the line which *apparently* joins the two lights (15647).



15614. Have prepared *two platina wire lights*, the platinum wires being $\frac{1}{100}$ of inch in diameter an[d] an inch long each. Have blackened all the wood surface within reach of the light from the wire so as to prevent extraneous light passing to the eye reflector.

15615. *Commutator* for inducteous helices and the two galvanometers. A board with 12 mercury cups as figured, the long channels being connected unchangeably with the galvanometers and the small cups also unchangeably, as represented, with the inducteous helices. The only moveable connexions are the four wires fixed to the cross bar handle. As these are brought forward or put backward they connect the galvanometers either with helices A and B or with them as B and A.



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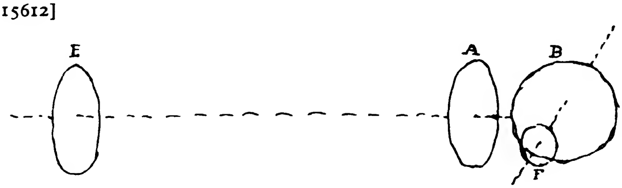
15616. *Regulator* for Electric current of Platinum lights. A cylinder of wood 2 inches in diameter and about   inches in length. A copper wire 0·035 of an inch in diameter is wound round it from end to end in a spiral of 63 convolutions, each of which

* [15612]



contains 6·35 inches, so that the whole length is 400 inches. There are screws for communication in the circuit at each end and one end has a sliding spring which, passing over the coils, presses on any one of them on which it rests and so can bring in successive lengths of the wire, and so increase the resistance sufficiently at pleasure.

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15617. Have made a new *flat helix*, the *third* one, of wire 0·1 of an inch in diameter. *Call it* L. It contains 50 convolutions; the outer contains 19 f. 6 inches and the inner 6 f. 6 inches—the medium is 13 feet, which multiplied by 50 gives 650 feet of copper wire of 0·1 diameter. Convolutions ½ an inch apart.

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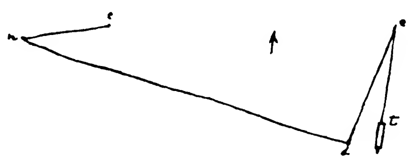
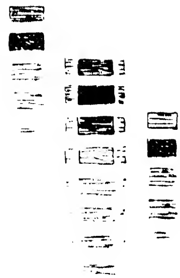
15618. The two galvanometers must stand in the same horizontal plane to have their connexions clear and bear approaching. Then by bringing one a little forwarder than the other, the needles may be brought to within 2 inches of each other apparently if necessary, and then if needful to shelter them, one shade would cover both. If a shade be used it must probably have a flat glass face to avoid reflexions.

15619*. Associated a candle flame at *c* with the needle reflector at *n* (15605) and 20½ inches about from it. The directing reflector *d* (15627), which is silvered glass, was placed 7 feet from the Galvr. needle *n*, and threw the ray on to *e*, being the eye-reflector (15598) of badly silvered glass and 32 inches from *d*; and the eye reflector returned the ray to the telescope *t* 25 inches from it (15613). All these reflections were in the horizontal plane or nearly so; the adjustment of the parts was easy enough.

15620. The telescope (15613) was focussed to give a good image of the Galvanometer needle *n*, for this gave the most compact and brightest object as light—the needle reflector then seemed full of light and was bright enough; it as an object having this form ▬, but the chief light was accompanied by many reflected lights. It had one above—then it had a series below, the nearest being as bright as the one above and the other[s] gradually diminishing in brightness. Also on the right and left of these were other lights

* [15619]





as figured, and which as regarding their place and degree of brilliancy, etc. deserve analyzing. Besides these on the right and left were faint repetitions of the chief column of lights, but those on the left raised and those on the right depressed—those on the left being the brightest. All these are no doubt the effect of the double surfaces of the reflectors *e* and *d*. They make the whole light as seen in the telescope a very diffuse object instead of an intense concentrated one.

15621. Using the opera glass (15647) in place of the telescope, the whole image was much smaller and more compact; it looked more like a spark and was of good brightness but had a burr form and appearance. On close examination the disposition of the parts of the light was seen to be as before. Must use reflectors with single faces and then probably the telescope will be better than the opera glass.

15622. Displaced the eye reflector at *e* and placed the telescope there, so as to look directly into *d*, thus removing the effect of the former and taking that of the latter only. Now the right and left reflexions disappeared and also those above and below the chief light, the image being bright and reduced to this form ; so we see what the double surface does (15634).

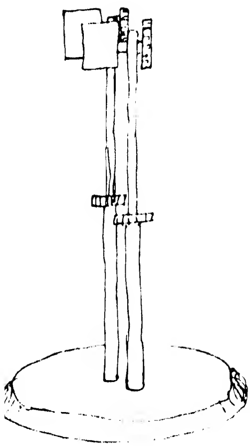
15623. I restored the eye reflector *e* (15622) but brought into action the plain polished silver surface (15598), so as to have a single surface there, but the form of the surface was so bad that the image could not be concentrated and was quite useless. Still, I think the extra reflexions of *e* were all gone. Put a steel button at *e*, but that gave as bad a result as the silver (15598).

15624. Restored the silvered glass of the eye reflector to its position at *e* (15619). Displaced the candle flame at *c* (15619) and placed a platinum wire ignited by a voltaic battery as the light there. When the effect was examined by the telescope at *t* (15619, 20), the object was found to have the same *form* and *size* and *reflexions* as the candle flame, but was not so bright. It could be made brighter by more current and was then better in that respect—but the thinness of the source in a horizontal direction did not give a better image in the telescope, with the present reflectors and needle. Theoretically however it ought to disappear quicker (15529).

15625. The current regulator (15616) performed its duty very well. Its wire, so much as was in the circuit, became hot, though the platinum wire not very bright. Lessened the resistance, then the heat of the platinum rose much but quickly fuzed, and this would be very liable with platinum lamps.

15626. I think I must use oil lamps at first and afterwards lime light if I have to give great velocity to the eye reflector.

15627. *Directing reflectors.* The two rays from the two galvanometer needles have to be so directed as to place the two images close together as one object, so that further observation may shew which is displaced first—it is easy by adjustment of the two lights to make the reflected rays from the galvanometer needles converge to one point, but after that they will separate again. I have had a pair of directing reflectors constructed, to receive the rays at this point, and reflecting them on in lines so nearly parallel that the two images shall appear close together when viewed by the eye or a telescope from any given point. A small plane silvered glass mirror is supported on a horizontal arm at the top of a little vertical column, and by a thumb screw can be turned on a horizontal axis; the vertical column is supported in a lower sliding vertical tube in which it can also turn on a vertical axis; the whole is set on a stand, and within an inch of it is a second like mirror supported exactly in the same manner. Now it is easy to place the stand so that the mirrors shall stand side by side or one overlap the other entirely or in any degree, as for instance thus*: and it is just as easy, when these directing reflectors are placed at the spot where the rays reflected from the galvanometer needles meet, to reflect these two rays in any direction by adjustment of the reflectors, and therefore to make them appear to the eye reflector or in the telescope as close together, giving the two lights as one object. After that, the point is to see which of these two lights disappears first. I have had such a pair of reflectors made and find I can place the two images of two separate candle[s] as close together as I like.



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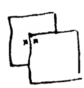
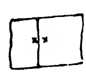
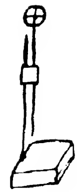
15628. *Guide rings* to determine the place of the ray. A ring $1\frac{1}{2}$ inch in diameter fixed on top of a straight wire passing through

* [15627]  or thus 

a cork—another wire fixed to a leaden foot passes through the same cork—by this means the ring is adjustible; cross wires are fixed to it and when a reflected ray is traced, the ring is set so as to encompass it and of course the direction of the ray is recorded for the time.



15629. *Light.* Microscope lamps, two. The flame of one of these lamps is $\frac{5}{8}$ of an inch diameter. Being placed 20 inches about from the galvr. needle reflector and the latter looked at from a distance of 6 feet, the effect was good, i.e. the needle was very bright; but the diffusion of the light to the right and left of the axis of the reflected beam was very great because of the size of the flame and the angle it subtended at the reflector. When the lamp was removed 6 feet from the needle, then the diffusion laterally was very much less--not a fourth of the former; but the light was weaker.

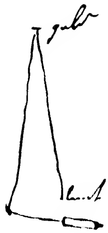
15630. The lamp flame 6 feet from the Galvr. needle and the telescope 7 or 8 feet from it, adjusted to give image of the reflector needle, gave a pretty good image, but not quite good, for needle reflector is not correct in its action. When a screen with a slit $2\frac{1}{2}/8$ of an inch wide was placed before the lamp, the light in the telescope was apparently as good as with the whole flame open. When the slit was $\frac{1}{8}$ of an inch wide the light in the telescope was much less, and with a slit of $\frac{1}{16}$ of an inch wide the light was still less—the diminution now being very considerable. Must place the lamp in the focus of $20\frac{1}{2}$ inches and then compare the effect of different sized slits. They ought not to *impare* the *maximum* brightness, but ou[gh]t to diminish the lateral diffusion.

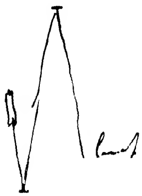
15631. I focussed the telescope for objects 12 feet distance—as that is the distance of the light—the light was poor, diffuse and bad, and I had to reduce the telescope to the focus distance of the needle reflector to make it good again. This ought to be so, as the needle reflector is not a plane mirror but concave.

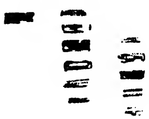
15632. I think the proper light will be a strong one, as the lamp or a lime light, at $20\frac{1}{2}$ inches and with a fine slit screen before it.

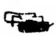
15633. As to the available angle at the needle reflector, I found I could easily make that of the incident and reflected ray 90° or even 120° and have very good light.

15634. Examined the image of the illuminated galvanometer









needle given by different reflectors employed as eye reflector and observed by the telescope adjusted so as to give most distinct image of the galvr. needle, the lamp being always 6 feet from the galvr. and the latter 6 feet from the reflector examined. The two directing reflectors (15627), of glass silvered, both gave this form of light  (15622). This was due to the inclination of the reflectors to the incident ray, for when one reflector was turned 90°, being retained in the same plane, the light still had the same position (not in relation to the turned reflector but) in relation to the plane of the incident and reflected rays. It is the double image of the two surfaced glass*. As the incident ray fall[s] more nearly perpendicularly on the reflector, the image becomes better and the two outlying portions gather up. If the glass surfaces were parallel and the incident ray perpendicular to them, then there would be no outlying portions and only a simple image. So the more direct the reflexion the better.

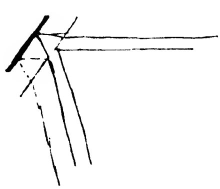
15635. The former glass of the eye reflector (15620) gave a very complicated image but without the lateral wings to the center portion. The silver surface (15623) gave as before a very bad diffused image. The steel surface (15623) gave as before a bad diffused image—but these were single images. Other steel surfaces (buttons) all with bad results (15598).

15636. Tried the four silvered glasses from the New Road workman ( ). The one professing to be flat gave three images far apart, and the chief or middle one very much confused by reduplication. The three concaves are of no use; first because of their bad surfaces and distorted action and image; next because, if good, they could not be used with the telescope but with the naked eye only. In fact a reflector there, if concave and perfect, would form its own instrument to an eye in the focus and would require no observing instrument, as the telescope—only a tube to guide and keep the eye.

15637. These reflectors, being parts of spheres, would confound two lights seen at once in them. If ever usable, the reflector here must be part of a cylinder. But probably they will not be wanted.

15638. So the reflectors, if of glass silvered, must be true and good in surface and the ray incident perpendicularly or nearly so. But

* [15634]



may probably obtain pieces of flat speculum metal—in perfect condition as to surface—that will serve far better. Try.

15639. Have cleared out the distilled water cellar, which has a stone floor: placed a table on it and the Galvr. A (15605) on the table:—then by a lamp and reflected ray observed the steadiness of the Galvr. needle and of the ray reflected from it, and found it perfect. Will do perfectly well in that respect.

15640*. I think the arrangement had better be made keeping the reflected ray in the same horizontal plane until it comes to the eye reflector—which revolving on a horizontal axis, will cause the images to move apparently in lines nearly vertical; and that the reflexions had better be as nearly vertical to the reflecting planes as may be.

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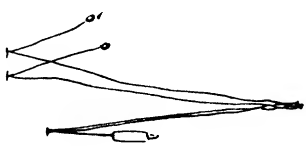
15641. Microscope lamp for light (15629). Copper chimneys pierced with slits are not good, as the apertures disturb the flame. Must have copper screens with slits outside of the glass chimneys.

15642. The whole lamp flame of $\frac{5}{8}$ of inch (15629) at $20\frac{1}{2}$ inches (focal length) from the needle reflector and the telescope 56 inches off. Great diffusion of the light; it appears on all sides of the telescope. The image of the reflector in the telescope is good and full of light, but when the needle is set vibrating in a manner strongly evident to an eye at the edge of the diffused ray, the appearance in the telescope or in the axis of the ray is as if the needle were immovable.

15643. The lamp only 12 inches from the needle reflector—gave a strong light but the ray still more diffuse and worse than before, though of the same kind (15642). Lamp at 24 inches distance—the effect better than in former case, but still when motion was seen at the edge of the diffused ray, it was not visible by any variation in the telescope unless the vibration of the needle were considerable. Lamp at 36 inches distance: as before, but in a less degree—the telescope would give an apparently fixed needle when it was really moving. When the lamp was 56 inches off, a portion of the same effect remained. As regards the perception of needle motion, the effect was better to the naked eye than with the telescope—the changes of the light in the moving reflector were more

* [15640]



distinctly seen. This diffusion is I believe chiefly due to the great size of the lamp flame. It may be partly to the imperfect polish and face of the needle reflector.

15644. The motion of the needle is no doubt vastly better seen by the reflected light than by looking simply at the needle as a whole: the motion was very manifest in the former way when the eye could not see it in the latter way.

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15645. Have ordered some *metallic reflectors* from Mr Varley (15682). Having written to Mr De la Rue, have received 7 or 8 specimens of plane polished reflecting metals from him: lumps about the size of nuts or wallnuts; but they may be cut.

15646. As edges of ground reflectors (needles or other) must be more or less rounded in the grinding, they must of course reflect light when the mirror is a little out of position. The remedy will be to break them off or else to stop edges by blackening them.

15647. The *telescope* (15613) magnifies about 11 times linear. The *opera glass* (15621) magnifies about three times linear.

15648. The lamps have been supplied with *copper screens*, having slits $\frac{1}{8}$, $\frac{3}{16}$ and $\frac{2}{8}$ of inch wide. They are blackened and allow no light to pass except through the particular slit pointed towards the Galvanometer.

15649. Lamp (15629, 48) with $\frac{2}{8}$ aperture, $20\frac{1}{2}$ (or focal distance) from the Galvr. needle: the telescope $54\frac{1}{2}$ inches between its object end and the galvr. needle: so the whole length of ray 75 inches. Arranged the telescope length for this distance: the needle image was of course obscure: but the lamp image was also confused in appearance . Shortening the telescope (as if for more distant objects) made this light larger and more indefinite at the edges: it was the confused image of the flame, and as the needle moved, it was cut off by its edge on the opposite sides alternately thus  .

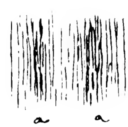
15650. Lengthened the telescope gradually so as to fit it for distinct view of objects more and more near. The light less and less and at the same time more and more bright until it was thus , being the distinct view of the needle. This was a good object and the best of those obtained: but as the image of the mirror was far

smaller than the image of the flame which would have appeared in a larger mirror, so the eye in the axis of the beam saw the light steady or fixed, whereas if applied at the side of the beam the reflector was seen to be in motion. There is no doubt that the needle is the object to be looked at distinctly, but the light ought not to subtend a large angle at it.

15651. Keeping all other things the same, the lamp screen was turned until the slit of $\frac{1}{8}$ was opposite the needle; this would subtend only half the angle of the former. The light reflected was very bright and good both to the naked eye and in the telescope. Moving either to the right or left, the reflected beam was found to be less diffused than the former; still, not much less; also it is not so bright–but have to remember that the surface of the steel reflector is not perfect and that probably much of the diffusion results from that.

15652. A ray may seem fixed in the telescope and opera glass which, when viewed by the naked eye, is seen to move. But in these cases both the telescope and opera glass shew that it is moving when they are moved to one side into the edge of the diffused ray.

15653. The small slit gives on the wall an illuminated surface of this character, being the camera image of the flame, in which the brighter parts *a*, *a*, result from the foreshortened projections of the sides of the flame. It is well to turn the shade on the lamp so that the brightest part falls upon the reflector.



15654. The lamp being at $20\frac{1}{2}$ inches and the $\frac{2}{8}$ slit towards the needle, a card screen $\frac{5}{8}$ wide across the middle of the object glass (which is $\frac{9}{8}$ clear) shut off nearly all the regularly reflected light, so that little more than half the width of the object glass transmits the ray of light. When the opera glass was used, a vertical screen of $\frac{6}{8}$ was almost enough to obliterate the image of the light seen through any part of the glass when adjusted to the place of the visible object. The opera glass is so large that it is easy to look aside of the screen, but then adjusting the screen, the effect recurs.

15655. Now moved the lamp to considerable distance, i.e. 54 inches, with $\frac{2}{8}$ slit–telescope as before, $54\frac{1}{2}$ inches–very good illumination. Reduced the light slit to $\frac{1}{8}$, using the brightest light (15653); still there was a very good light in the axis of the beam,

but there was much less diffusion than before, so that the motion of the needle tells quickly in the telescope. The image of the needle seems fixed but the image of the reflected light travels across it.

15656. The opera glass gives with this arrangement an excellent little luminous image of the mirror, which by motion of the opera glass describes good curved lines of light.

15657. As to a *flat needle reflector*. Took one of De la Rue's flat reflectors (15645)—blackened it over—cleared off four places of about the size marked, by a point of wood—fixed it before the galvanometer needle as a stationary object of four differently sized flat reflectors placed the lamp with $\frac{1}{8}$ slit aperture at 8 inches from it and the telescope at the distance of 54 inches as before. To the naked eye the reflexion was very bright:—the light in the different reflecting parts built up one image of the flame and slit, the eye adjusting itself to the flame distance. There was much diffusion of the reflected ray and of course more in a vertical than in a horizontal direction.

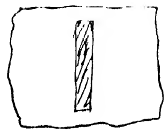
15658. The *telescope* was adjusted to the reflector distance—all the four light images were good but the largest of course the most prominent and effectual. Shortened the telescope so as to adjust it to the lamp distance: this was a disadvantage, for the image of each reflecting place was enlarged and became blurred and the light became diffuse with it. Using the *opera glass* adjusted to the reflecting surfaces' distance, the images were very good—small, compressed but very bright—the largest not too large, quite starlike to the eye. When the glass was moved, the lines described were very fine and good: the largest spot perhaps the best line but the smallest was good. I could not make the lines invisible by the quickest motion the hands could give. The effects were very good.

15659. Now the lamp with $\frac{2}{8}$ aperture the full distance of 54 inches. To the *eye*, the effect in the axis of the beam was good, but the diffusion, especially in the horizontal direction, far less than before—the effect star like. In the *telescope* adjusted to the reflector, the effect was very good. The largest reflecting surface makes most impression on the eye. On shortening the telescope, the images became worse, ran together and at last obtained the

image of the slit and flame, losing that of the reflectors: but all these effects were worse than those with adjustment to the reflectors. The distance thus *experimentally* obtained was found to be right for an object 108 inches off. Now the distance from the lamp to the reflector and then to the telescope was 104 inches, so the actions and results coincide. But it is best to adjust to the reflector, which is the thing whose motions are to be observed. The *opera glass* gave an excellent star like effect—with very little diffusion. When it was moved, the light lines were very good.

15660. Employed the $\frac{1}{8}$ lamp slit to the eye and to the opera glass the effect was very star like and good.

15661. Now employed a large De la Rue reflector (15645), the angle subtended by it being much larger than that of the flame of the $\frac{1}{8}$ slit—the lamp was at the full distance of 54 inches. To the eye the whole flame is in the reflector and of course travels across it as the eye moves right or left. *Telescope* up, adjusted to the reflector, gave it clear and a round confused light in it. Adjusted to the lamp light, it gave it smaller and more brilliant and distinct, and the reflector obscure. This is perhaps the best when the reflector includes the whole of the light, but the worst when the reflector subtends much smaller angle than the light. When the *opera glass* was used, the adjustment to the light was also the best. Whatever becomes the luminous object, either the whole light or the whole reflector, should be made distinct in the instrument.



15662. Have a concave silvered glass reflector, radius about 27 inches, i.e. when a candle flame and its image are equidistant, 27 inches is their distance from the reflector. Of course a lamp being in the distance and the mirror moved, the image traverses in one direction if the eye is near the mirror and in the other if the eye be outside the secondary focus. The image of the flame is largest when the eye is in the secondary focus and diminishes as the eye advances or receeds from that position, but at any distance within those I should use, it is *always* larger than the image would be in a plane mirror. When the eye is close up to the concave mirror, the nearest approach to equality is obtained. Such is the case for all distances of the flame or object.

15663. Theoretically, a concave reflector with a small light in one

focus and the eye in the other would give the most rapid passage of light, but if the light subtend a larger angle than the reflector, as is my case, then a plane mirror should give quicker disappearances than the concave one.

15664. Inasmuch as the light by removal to a distance does not sensibly diminish in apparent intensity but only in area, so as long as the light subtends an angle at the eye equal to that of the reflector, a *plane reflector* will probably be best for the needle.

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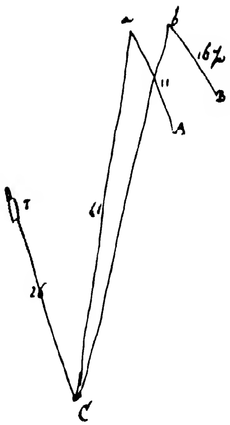
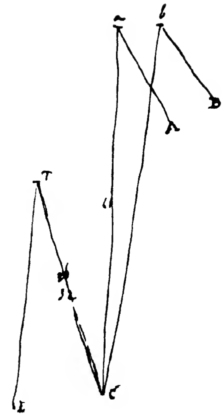


15665. A and B are the two Argand lamps with the $\frac{1}{8}$ of inch slit in the dark shades—*a* and *b* are two of De la Rue's metallic reflectors (15645), with small reflecting faces opened in the blackened surface about $\frac{1}{10}$ of inch long, like the needle reflectors—C is the double object reflectors (15627), the reflectors being glass silvered—T is the telescope—B is $16\frac{1}{2}$ inches from *b*, and *kept constant* A is 11 inches from *a* and the effect in the axis of the ray is equal to that of B—both rays were directed by the position of the lamps through the center of a ring at C and compared there both by the eye and the telescope. Then A was removed to 42 inches, or nearly four times the first distance. Now the two objects *a* and *b* both appeared in the telescope of the same size as before, but *a* had lost in brilliancy, though it was still a very good image. So the lights had better be near.

15666. Left the two lamps A and B and the two fixed reflectors *a* and *b* as they were, sending the reflected rays to the ring at C. Then replaced that ring by the directive or object reflectors (15627) of which the reflectors are silvered glass, and taking one ray in each reflector, sent them on to the eye or telescope at T, the angle between them being purposely so small that the two lights appeared as a double object in the telescope at once. This required much adjustment, but at last was done very well. The two lights appeared very well both to the eye and in the telescope and also in the opera glass. The distance from *a* to C was 41 inches and from C to the telescope 26 inches.

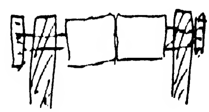
15667. I now added the eye reflector (15598) only, glass silvered, at T, 32 inches from C or double object reflectors, and then

applied the eye or telescope at E, 4 feet from T; found good; two lights well together in the reflector T–and the same result was obtained 6 feet off. The arrangement will do. The opera glass at E, 6 feet from T, gave a very good image, i.e. close, star-like and bright. I believe that the large concave needle reflector and the lamps near at hand will do very well.



15668. I placed one of De la Rue's metal reflectors (15645) at T and obtained an excellent image of the two lights as one double object, either with the eye or the telescope or the opera glass. Even when T was placed more oblique, so as to make the angle of the reflected ray 90°, the effect was good, and as the eye could then be taken 24 feet from the reflector, it was found that a good image could be given even there.

15669. I want now a new *directing set of reflectors* (15627). The reflectors must be metallic, not of glass. They must be in one plane, meeting at their reflecting edges. They must adjust by long levers or even a screw action. Their friction must be stiffer. They should not be behind each other but equidistant from the needle reflectors, or else the latter cannot be brought both into focus at once. The adjustments should all tend to bring the two lights side by side on a horizontal plane.



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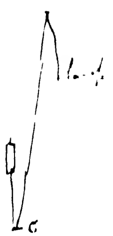
15670. Prepared a De la Rue reflector (15645, 57), equal in surface to the concave steel needle employed heretofore (15605) of $20\frac{1}{2}$ inches focus, to ascertain whether the concave had any advantage at different distances over the plane reflector. Arranged the lamp with $\frac{1}{8}$ slit (15648) at $20\frac{1}{2}$ inches from the reflectors, they being one over the other so as to throw the images very near to each other as regarded the eye. Examined the results by the eye, the telescope and the opera glass at 5 feet distance–the effect of each was as far as I could judge very nearly alike. Then placed the lamp 5 feet off, or three times the former distance, and observed the images as before–the flat one or De la Rue's was the best. It would seem therefore that it is not concavity of needle that is wanted but *perfect polish.*

15671. Examined a Galvanometer in place and for that employed Becker's instrument C (15603), with its reflecting needle of $20\frac{1}{2}$

inches focus hung in the middle of the coil, associating a single pair of zinc and platina plates separated by wet paper, and so adjusted that the circuit was completed by making contact of the end of one copper wire either with the wet paper on the zinc or with the platinum on the wet paper. The lamp with slit of $\frac{1}{8}$ of an inch was placed $20\frac{1}{2}$ inches off, the eye and telescope being 5 feet off. The light was well visible in the needle whilst it was quiescent. The shortest tap of the end of the copper wire on either the wet paper or the platinum instantly sent the light out of sight, and when the touch was on the platinum the jump out was very sharp and sudden. As the needle swung to and fro to rest, the line of light was dim, and I suspect I shall ultimately require the lime light with high velocity of moving eye mirror.



15672. In respect of the line of light described in the moving eye reflector (15545) when the needle is still—employed the arrangement just described (15671), but placed the eye reflector at C, 5 feet off—it was furnished however with only a bad glass reflector (15636), which gave a broken image in the telescope or eye 18 inches from it. When the contact maker C was on the catch and the eye so placed that on loosening it the mirror, urged by the spring of the instrument, should revolve on the axis and form a line of light apparent to the fixed eye, it was found that that light was very poor; but I do not know with what velocity the mirror was turning; probably not more than with 2 or 3 revolutions per second. This would seem to imply that the lime lights may be wanted.

15673. The *telescope* was of no use here because of the badness of the reflector. The *opera glass* was better because the image more compressed and brighter.

15674. The iron spring of the *contact maker* does not seem to affect the galvanometer needle by the necessary changes of its position at the distance of 5 or even 4 or 2 feet off.

15675. When the eye was so placed that the contact stop brought up the reflector on its axis so that the luminous image was visible to the eye, the image did not seem to go beyond that point, but the line of light terminated and stopped in it. So far as this shews that if the needle moved at that moment the light would disappear in view, it is well; but it also shews that it would not be the

moving but the fixed lights which would disappear and that is not what is wanted. The apparatus must be such that the mirror moves *after* the contact is made.

15676. Tapping or pressing on the Galvanometer table is bad and greatly disturbs the needles. The galvanometers must probably have a table to themselves on the stone floor.

15677. The Galvanometer needles will probably require to be shaded from the wind, but perhaps not in front. The helix itself will probably make a cell.

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15678. Degree of inclination of 2 lights to the horizon, visible. Cut two notches in two black cards so that they could be placed before the lamp (15629) at any distance apart. They were about $\frac{1}{30}$th inch wide—$\frac{1}{10}$ long horizontally—about one tenth apart horizontally, and the upper edge of one level with the lower edge of the other, so that they were $\frac{1}{30}$ of inch apart in a vertical direction at either their lower or upper edges.

15679. The *eye* could see their inclination as long as the two lights were distinct. With the *telescope*, the inclination could be seen at the distance of either 4 or 35 feet or at any intervening distance. With the *opera glass* the inclination was distinct up to distance of 24 feet; then the lights became starlight and their separation and inclination doubtful, and quite doubtful or bad at 35 feet. At 12 feet or less, the lights were very bright and the *inclination* very clear.

15680. Made a quarter difference in vertical separation, i.e. $\frac{2}{30}$ between the upper edges of the apertures. The *eye* could see the inclination up to a distance of 13 feet, but then the two lights by irradiation blended. The *opera glass* could distinguish the inclination up to 30 feet distance, then the lights combined. The telescope shewed the inclination well at 35 feet distance, which was the greatest distance I could arrange.

15681. Made the light dimmer thus: the distance between the lamp and the pierced screens (15678) was three inches—placed a sheet of foolscap paper against the screens on the lamp side and made it the visible light. Now the unassisted eye could distinguish the two lights and their inclination up to a distance of 16 feet in

a moderately obscure room (the lower apparatus room). The *opera glass* could shew the lights and their inclination at a distance of 35 feet, and far better at these distances than the more brilliant lights because irradiation does not confuse them. Therefore the weakening of bright lights by motion will to a certain extent be an advantage. The *telescope* could also shew the inclination at the extreme distance of 35 feet, but the light was then very feeble—far too feeble to allow of dilution by motion.

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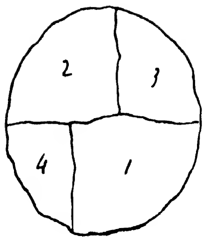

15682. *Reflectors.* Mr Varley has ground me a flat speculum metal reflector and broken it into four pieces, size[1] and shape as in margin. When examined by an eye glass and reflexion of a distant window frame, the surface seems perfect. Mr Becker has also polished me a *hard steel button*; but examined in like manner, its surface is striated, being not perfect in form though well polished. Examined these thus.

15683. Put up the lamp and the two apertures (15678). Placed one of De la Rue's reflectors (15657) which had not a good surface (15682) 32 inches from it, so to reflect the rays at an angle of 90° about, and then examined the figure of the object as seen by the telescope 12 feet off. The image of the two holes when most perfect was pretty good, but at the best was somewhat distorted. When Becker's steel button (15682) was made the reflector, the two images at 12 feet were at their best; very star like at their best, not bein[g] clear defined objects. At 27 feet they were also starlight, but not definite in form. A piece of the Varley mirror (15682) being put up was best of all and very good—forms excellent.

15684. *Galvanometer.* Mr Becker has made me another galvanometer like the former (15450: C); the present is to be called D. It contains about 180 feet of covered copper wire of an inch in diameter in about 500 convolutions. I have papered the inside of this and of galvanometer. Have also made two black paper shades for the coils and needles of C and D, which I hope will sufficiently shelter the needles without interposing glass in the way of the ray. D is altogether like C in all points.

[1] Reduced to $\frac{3}{4}$ scale.

15685. *Time.* It would appear very hopeless to find the time in Magnetic action if it at all approached to the time of light, which is about 190,000 miles in a second, or that of Electricity in copper wire, which approximates to the former. But then powers which act on interposed media are known to vary and sometimes wonderful[ly]. Thus the time of action at a distance by conduction is wonderfully different for electricity in copper, water and wax. Nor is it likely that the paramagnetic body oxygen can exist in the air and not retard the transmission of the magnetism. At least such is my hope.

15686. As to its detection: a difference of $\frac{1}{30}$ of an inch can be seen with a radius say of 10 feet, and it is the $\frac{1}{22620}$ part. Suppose we say that the light lines will be visible with a revolution of the contact mirror thrice in a second; that is equivalent to a revolution of the light ray 6 times in a second, so that $22620 \times 6 = 135720$; so that the space moved through in the $\frac{1}{135720}$ of a second will probably be easily distinguished. If that be the time for induction through 100 feet of distance, it would shew a transmission of magnetic power with a velocity of 135720×100 or 13572000 feet in a second or 2574 miles, or about $\frac{1}{74}$ part of that of light. Probably the radius may be doubled or trippled–perhaps the rotation be much increased, but then the difficulty will be to catch the moment of cessation, for the impression of the preceeding lines of light will remain on the eye if the revolutions are more than ten in a second.

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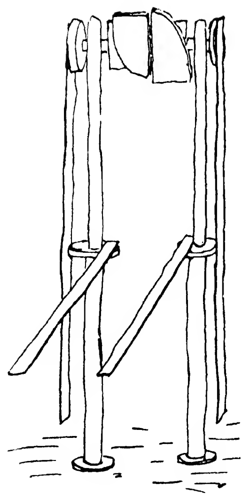
15687. *Lamps* (15629, 48). Have supplied each with a tripod base, the feet being screws for minute adjustment of the height.

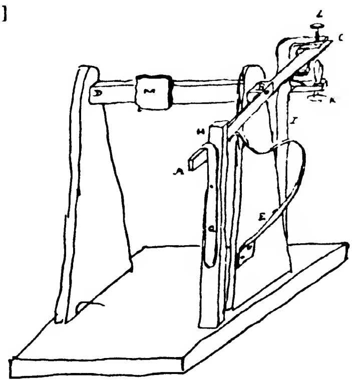
15688. *Directing or double reflectors* (15627, 69). Have set the two uprights farther apart, have fixed on the two Varley reflectors Nos. 3 and 4 (15682) so that they can stand in the same plane and yet move freely for adjustment–have blackened all the parts near the reflectors; have put long lever handles both to the horizontal axes and to the vertical axes, so as to allow of a slow and steady adjustment, and made all ready for practical use.



15689*. The *contact maker* carries the eye mirror, i.e. the revolving mirror (   ), and it is intended to allow of such adjustment as to make contact with the inductric spirals exactly at that moment when the mirror on it is in the position to shew the illuminated stationary galvanometer needles. Then the point is to ascertain which image of the two sent by the double ref[l]ectors disappears first. A B C is a metal lever fixed on the horizontal axis B D—on this axis is fixed the Varley reflector M (15682) No. 2, and all these parts move together. A spring E forces up the A end of the lever but a catch G holds it down. There is a pin at H to stop the ascending lever when that is necessary. From A to B the lever is flat, with its edge upwds.; from B to C it is flat with its edge horizontal; I is a strong brass support which carries two adjusting screws K and L. When the catch G is set free, the lever end C falls upon the top of K and makes battery contact there, for one wire of the battery is fixed into B and the other wire into the foot of I. Now by adjusting the screw K, this contact can be made to agree with the proper position of the reflector M, so as to obtain the desired result. Only, as the screw K brings the lever and axis to a stand still at the moment the contact is made, and as the motion of the mirror should be continued at that instant, so an additional point of contact is added on a spring within the arms K and L; the point is on a part of the spring close by the end of K and the upper part of the spring is governed by L. In this way the new point N makes the contact and continues it, whilst the lever still moves until it is brought up by K. All this implies that the motion may cease at that time, but probably other principles dependant upon the duration of impressions on the eye may come into play. All the parts near the reflector were blackened.

15690. As to the *radius of the ray* reflected from the revolving mirror and therefore the circumference of the circle on which the

* [15689]



moving image travels, when the eye was at a constant distance from the reflector (15689) and a luminous point was the object looked at, whether that point was 2 feet or 6 feet from the reflector, the motion of the latter through a given arc caused the object to traverse exactly the same extent across the mirror, and therefore it moved through the same angle in both cases. This of course would be so, for near and distant objects would all move as parts of the same picture, suffering no relative displacement. But though the angular distance of motion is the same, the measurement of the angle by a standard would not be so; an inch would measure off three times as many degrees on the smaller circle as on the large one. So that distance of the light increases the sensibility without increase of velocity.

15691. When the light was fixed as to distance from the mirror and the eye removed to different distances from the mirror, the apparent space across or upon the mirror traversed by the luminous point increased exactly with the increase of distance; but as the angular width of that part of the mirror diminished exactly in the same proportion, so the apparent angular travelling of the luminous object would be, as before, the same for all distances; but as the object distance is increased, so the sensibility is increased, without any increase of velocity.

15692. The distance to be measured therefore as that which gives the radius of the circle through which the object apparently revolves, is the distance of the revolving reflector from the eye added to its distance from the object: and it consists with convenience to place the revolving reflector and its accompanying discharger at any distance between the two, and so nearer to one than the other, as may be convenient.

15693. The duration of light on the eye is one tenth of a second. So if the revolving reflector turn ten times in a second, equivalent to 20 revolutions of the reflected ray, the light will seem to be continuous. The question then is, can the successive pulsations be distinguished? Or having the two lines of light at once before the eye of equal intensity, and supposing that one disappeared before the other, would a corresponding difference in the luminosity of the two lines be evident before both disappeared (   )?

15694. Mounted a plate (polished) cut off from one of De la Rue's

reflectors (15645) on a revolving apparatus so as to be able to examine the effect of duration on the eye.

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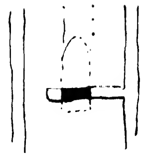
15695. The small pulley is 1·4 inches in diameter and the large one 17·5 inches—so that one revolution of the latter = about $12\frac{1}{2}$ revolutions of the former.

15696. Put up a lamp light with the slit of $\frac{2}{8}$ wide (15648), the visible part of the flame being about half an inch high, and looked at it in the mounted mirror (15694) about 10 feet off. Of course, when still the image was either in or out of the mirror, and if in, was very bright. When the mirror was revolved very slowly the image appeared bright, at long intervals, once for each revolution; when the revolutions were quicker, the light was less bright but recurred more frequently as a line crossing the whole of the mirror—as the revolutions were more rapid, the recurrence of the light occurred almost as soon as the previous impression had faded from the eye, and then the light was a pulsating intermitting effect. With quicker revolution, the light appeared as a pulsating *continuous* effect, and as the velocity continually increased, the light at last appeared a continuous equable line of dull light, due to recurring images of the light. As the mirror was about 1 inch broad, the light would recur each $\frac{1}{720}$ part of the time of its revolution, so that it was visible for that time only, but the number of revolutions was great enough to make this a continuous effect on the eye. The constant equable light is not quite so bright apparently to the eye as the quivering continuous light which preceeds it.

15697. Six revolutions of the mirror in a second—the eye just able to see that the light was out between each. Twelve revolutions in a second—the light apparently continuous but intermitting. Twenty-four revolutions in a second—the light appeared uniform to the eye. With higher velocities the luminosity of the apparent light sank a little (15696), shewing that it was intermitting in brightness after the eye ceased to perceive that it was so by apparent variation. The eye cannot distinguish little differences here well—or carry them in remembrance. It would

probably be difficult to tell whether, of two traces side by side, one was a *little* weaker than the other.



15698. Diminished the size of the luminous object by putting up a black screen with a horizontal slight [? slit] $\frac{1}{12}$ of an inch wide, so that the luminous object was $\frac{1}{4}$ wide and $\frac{1}{12}$ high. All the effects were as before but much weaker. With quick velocity, the constant line of light was produced, but very poor as an object. In fact, $\frac{1}{12}$ of an inch is only $\frac{1}{9000}$ of the circumference of a circle of 10 feet radius, so that the intensity of this apparent constant light was only $\frac{1}{9000}$ part of the bright fixed image that was given in the unmoving mirror.

15699. Made the light slit $\frac{1}{8}$ wide by half an inch high. The effects like those with the $\frac{2}{8}$ slit, but feebler, as might be expected ( ). Put on the cross slit of $\frac{1}{12}$ (15698). All the effects much reduced and now the constant light with high velocity very feeble indeed.

15700. Took a De la Rue reflector (15645) blackened; cleared two reflecting places about the size of the galvanometer reflecting needles—threw the lamp light on to them and directed the reflected rays to the revolving mirror (15696). When quiescent, there was a good star light in the mirror, but when in revolution the light disappeared or nearly so. In a perfectly dark room it would be better, but not good enough for observation. If the lime light were employed in place of the lamp, then the lines seen in the moving mirror would be probably distinct and bright enough.

15701. In order to obtain some coarse practical effects with this revolving mirror and a continuous light, I arranged the two lamps with their flames side by side about an inch apart, with the slits of $\frac{1}{8}$ of an inch, in use, and half an inch high. Of course the appearance and effects were as before, except that two lights or lines of light were visible. When both lines were steady and not quivering (15696), I let a screen fall sideways at the lamps, so that it should occult one before the other. Well, the corresponding lines were put out in succession in right order, but the judgment was in no respect aided, perhaps not so sure as if one looked directly at the lamps. The same was the case when the velocity of the wheel was less and the lines of light were quivering—there was no power of distinguishing the maximum places of light in the lines and comparing them instead of the whole of the lines.

15702. Occulted the flames by letting the screen formed with a inclined lower edge fall from above. The continuance of one ligl longer than the other was evident in the mirror, but not more s than by looking directly at the lights.

15703. I do not see in all this, or by theory, how a continuou light is to help me in very short periods of time. It has th advantage, by rapid revolution of the mirror, of giving a con tinuous image, because of the retention of the light on the eye but if the difference of time at the galvanometer was the $\frac{1}{100}$ c the $\frac{1}{50}$ or the $\frac{1}{10}$th part of the time of a revolution of the mirroi or even if it were as great as the time of a whole revolution, sti if that was at the rate of 50 or 100 per second, the eye would nc be able to perceive the falling in character of one light first, becaus of the duration of the impression of both on the eye for the tent of a second. If the time to be measured between the termination of the two lights were long, as the tenth of a second or even muc less, then indeed constant light might be used, i.e. by employin the lever apparatus, in which the lever moves with far less velocity and causing the galvanometer needles to move at the momer when the reflected lights are in the mirror. But it is not likely the Magnetic time for 100 feet of interval will be long enough fc such an observation—yet try.

15704. Supposing we should find the time for 100 feet the $\frac{1}{10}$ c a second or more, then we might separate the luminous appearance but it would require that the mirror should shew the needles i that moment when one had moved and not the other; the ne› revolution would then not repeat the luminous impression.

15705. In contrast with the *constant* light is the *momentary* ligh as the *electric spark*, which has a fixed and definite place in th revolving mirror—but then the difficulty is to make it appear i the mirror. The contact maker is intended to perform this—bu then it will do it only for slow velocities and with a fixed light to do it for high velocities and for a momentary light would b very difficult. Must look at Foucault's expedient.

15706. For my purpose too, it would require that one spark ligl should serve to illuminate both galvanometers, or else the simu taneity of light at the commencement would not be secured. Th may be arranged.

15707. Then to secure an apparition of the light more frequently, I want a recurring electric spark. For this purpose employed a Ruhmkorf apparatus with a Leyden Jar attached and cause[d] the spark to appear between platinum terminations. Now this spark, which is free enough at first, soon ceases or becomes more difficult, and I must search for the cause of this effect. By holding a feeble spirit lamp flame against the termination of the spark wires—the spark looses its snappishness and become[s] diffuse, dull and quiet, continuing to go on; but by carrying the flame along the wire a little way from the end, the bright sharp sparks were renewed and could be obtained continuously. Is heat carried over here?

15708. Put this spark apparatus in the place of the lamp. The stream of sparks was very rapid and continuous. When the reflector (15696) was revolved slowly, they appeared as a line of sparks; but when very rapidly, they only occasionally came into the mirror, were very distinct and starlight, having no sensible prolongation, and were just what I wanted if I could keep them in sight. They were not single sparks but each discharge consisted of a larger and several smaller ones*, recurring in groups at intervals, and though the large intervals between spark and spark were quite irregular, those between the different spark[s] of one discharge were alike.

15709. The stream of sparks from the Ruhmkorf without the Leyden phial—or if the phial be associated, those obtained by the use of the spirit lamp at the place—are continuous streams of intermitting prolonged electric discharges, and if such a stream can be in any way used, they may be well obtained this way. Up to a certain velocity of rotation, such stream of *intermitting light* may be useful. If so, then the ordinary *voltaic light* will probably be found to be such a stream, especially when it is accompanied with noise. Even a constant flame light, if reflected from a mirror on a spring and that connected with a sounding apparatus, as an organ pipe or vibrating rod, will give a like intermitting stream. The double appearance of such a stream in the revolving mirror would shew one series of dots advanced a little before the other if the time of motion of one galvanometer needle was a little after the other.

* [15708]

15710. Had an accident with the Ruhmkorf by letting it fall, and sent it to Mr Ladd to repair.

6 OCTR. 1857.

15711*. Use and adjustment of the *contact maker* (15689). L was the argand lamp with the slit of $\frac{3}{8}$ of an inch; G the galvanometer and its reflecting needle about 18 inches from L. The eye mirror and conjoined contact maker was at B, about 6 feet from G, and the eye or telescope at T, about a foot or 18 inches from B. The galvanometer G was quiescent and so the ray reflected steadfast. Now the motion needed for breaking and making contact at B moved the mirror there, and the reflected image of the needle G moved in a vertical line across it. If the eye were raised or lowered, it was easy to find a place for it where the first contact was completed when the image of the light was in the *middle* of reflector B, after which it travelled on and out of the mirror, the lever moving until brought up by the final stiff stop. This of course is the adjustment which I require when the galvanometers are in use. When the eye was made a *fixture*, and that is needful in using the telescope, I could effect the same adjustment by tilting the contact maker bodily with a wedge—or for finer adjustment by setting the screws and contacts at L (15689).

15712. Now connected a single pair of zinc and platina plates, with wet paper between, with the Galvanometer and the contact maker. All was right, i.e. as long as contact was not made the Galvanometer needle was steady; but when contact was made the needle instantly moved and the light image was thrown out of the contact reflector. Now proceeded with the Voltaic pair in circuit to make contact by letting the lever off from the catch, beginning with a quiescent galvanometer needle each time, and then by trial found a place for the eye at which the light line was continued unchanged to the middle of the moving reflector of the contact maker, but disappeared for the other half, shewing that contact occurred when the luminous image was in the middle of the visible line. Now this disappearance was not sudden but gradual, the gradation extending over half the reflector and shewing what *time* was occupied in the disappearance, which time is against me. When the voltaic circuit was thrown out of connexion, it was

* [15711]

easy to see the difference between the permanent line then produced and this transient or dissolving line; and perhaps if two lines are placed side by side, the difference in time may be apparent.

15713. This *time* may be affected in two ways or more. It will depend in part upon the slowness of motion of the Galvanometer needle, and that may depend in part upon the weakness of the current—but the current is quite as strong and stronger than any I am likely to have by induction. It will also depend upon the size of the light and its nearness to the Galvanometer needle, which in the present case are against a quick indication. A lime or voltaic light at a distance would be better in that respect.

15714. Trials to observe with the telescope and the opera glass did not give me good effects as regards the line of light, but the brass spring of the contact maker had gone wrong and I could not adjust properly so as to make any useful observations.

15715. Must soon resume this subject.

15716. Sundry points to think over: 15658, 80, 92, 701, 3, 4.

15717. Focus for compact image object: 15584, 6, 90, 2.

15718. Continuous light object: 15693–704, 12, 3. 15703 only useful for long times.

15719. Momentary light: 15705–9.

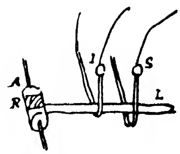
15720. Continuous light is objectionable–because it makes an apparent line across the moving eye mirror, not a spot as an electric spark would do because as eye sensation endures for $\frac{1}{10}$ of a second, if the eye mirror revolved at shorter intervals, the sensation at the eye would be continuous–because if the retardation of one galvanometer behind the other was only $\frac{1}{100}$ or $\frac{1}{1000}$ of a second, the difference in displacement of the galvanometer needle images would not be visible in two light lines enduring each for the $\frac{1}{10}$ of a second, though it might be if the light lines were replaced by two electric spark images, etc. etc. See 15703– and indeed 15693–704, 12, 3.

15721. A momentary light, as an electric spark, if produced at the right time, would mark a spot in the revolving eye mirror and not a line–and two spots from the two galvanometers would shew a very small relative displacement (15701, 2), and perhaps shew a difference in the galvanometer *time*, even if both were sluggish, provided they differed. If they did not recur, the differences of the two spots might be well observed; if they did recur, still the series of spots side by side might shew the time difference–because the electric spark light, being instantaneous, would appear in the revolving mirror as bright and sharp as it would do in the fixed mirror, whereas the continuous light is very seriously diminished in the intensity of its light by motion of the mirror (15696–700).

15722. What is wanted *at a given moment* is the *making* or *breaking* of a voltaic current to originate the magnetic induction, and in this respect probably the breaking of the current is the best (   ), from the circumstance of the action being then sharpest–also the *motion* of the eye mirror to watch the deflection

of the galvanometer needle—and also an *electric spark* to illuminate the galvanometer needles and produce the object seen in the eye piece. Now in principle these three may I think be combined. The eye mirror and the contact *maker* or *breaker* may be so combined, as before described (15689, 711, 2), as to give the moment of induction and the position of the moving eye mirror such as is required for the observation of the action on the galvanometer needles: and further, it seems possible by using the *interruption* of the current as the source of magnetic induction, to obtain a spark there, at the right moment for observation. The only difficulty seems to be that the spark so obtained must be simultaneous with the beginning of the inductive action and so anterior to the motion of the galvanometer needles, and therefore too soon to shew their motion. But if the spark, by igniting the charcoal or zinc or other matter at the place of disjunction, is prolonged only the $\frac{1}{1000}$ of a second, it will probably be in time; and if not in time, it does not seem difficult to arrange another battery and place of disjunction, so that the motion of the eye mirror should produce the spark there *in time*. An essential point I think would be that the same spark should illuminate both needle reflectors, or else the apparent difference of their places could never be trusted as a measure of time (15706).

15723. Suppose A an axis, carrying R an eye reflector; L a lever fixed to the axis A; I the terminals of the inductric induction current and S the terminals of the spark current, the bent ends being springs to allow of the opening of these terminals by the descent of the lever L. Then these may be so adjusted that the inductric current may be opened at any given distance, in time however small, before the production of the spark by the opening of the spark current; and the motion of the lever, before it opens either of these currents, will be that which sets the eye mirror in motion and will allow of its place being accurately adjusted to the moment of time when the reflected spark of S is to be seen in the middle of the mirror R. There seems to be no difficulty in arranging all this practically, by good workmanship.



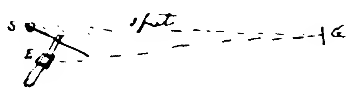
15724. Worked on possibility of using disjunction spark as a source of light (15722), the moving eye mirror being on the apparatus by which the disjunction is to be effected. The apparatus (15689) was employed—but the *additional* point of contact on a spring there referred to but not lettered is taken away. So the lever B C makes contact with L and with K if needful; when the part H is under the catch H, the lever at C is in contact with screw L, and one wire of a battery being connected with it at B and the other wire with I, separation of C from L breaks the connexion and occasion[s] a spark which is to act as the light. If the contact be broken by bringing C down by the finger (the arm C being elastic enough to allow that), then the spark is produced whilst the eye mirror M is stationary; or if contact be broken by letting off the catch G H, the mirror M is in motion at the time the spark is produced.

15725*. Arranged the contact breaker about 3 feet of[f] from G—S is the place of disjunction where the spark appears; E is the eye mirror either fixed or moving as desired (15724) and G represent[s] the Galvanometer reflector: at present it is one of De la Rue's reflector[s], nearly an inch square, fixed—and so adjusted as to reflect the ray from the spark S on to the inclined eye mirror at E and allow of its observation. A battery of 10 pr. Grove's plates was connected with the contact breaker, the surfaces of contact at the place of disjunction S being of brass—the connecting wires were about $\frac{1}{10}$ of an inch thick and 54 feet in length, that the battery fumes might be away from the apparatus.

15726. The breaking spark, looked at directly about a foot off, was a good bright object, very star like—producing irradiation and some degree of linear extension, appearing about $\frac{1}{15}$ of an inch long. Being looked at in E after reflexion from G, the distance of the spark object being then about 7 times what it was before, it was much feebler but of the same character; it appeared fixed, giving irradiation, having no sensible elongation, and made a very good object. The mirror E was fixed, the contact at S being broken by the hand.

15727. Now broke contact by the catch (15724) so that mirror E might move at time of the spark. The spark kept its place generally—but was drawn out somewhat in length, perhaps to twice

* [15725]



what it was when mirror was still, and it was brighter at the bottom than at the top part. The upper part is that of the first opening, and it elongates downward, both in reality and to the eye; the lower end is the brighter part. *It ceases suddenly* and the effect serves well to mark the place of the spark as a luminous object. The effect of time in the spark very evident—even with this comparatively slow velocity of the mirror E, the *time* of the spark is shewn by the elongation.

15728. With the gas full up, the place of the reflector G is well seen and so the eye can be guided by its place—still the spark is well seen in the reflector E because of its brightness: however, the gas light might be down and the reflector G rendered visible by a small lamp near at hand.

15729. The above effects were with a battery of 10 pr. of plates (15725). With 2 pair of plates the spark was very poor and uncertain. With 5 pair it was bad also. With 8 pair it was certain and good. There is too much obstruction at points of contact and probably part of the effect of time (15727) due to this. Must have the zincs and platinums soldered together—the screw contacts converted into Mercury contact—and where screws perhaps essential for adjustment, as at S, perhaps amalgamate the threads.

15730. The *great cylinder helix* (without its core) put into the battery circuit—used 5 pair of plates—the breaking spark poor though better than without the helix. Introduced a soft iron cylinder 1 inch diameter and 12 inches long: the effect very little better. Introduced its own large core a little way—not better—put in the whole core, then the spark pretty good.

15731. It is not enough to bring the surfaces at S in contact to produce a spark on breaking contact—the oxide from previous sparks often prevents contact—or sometimes the obstruction is not enough to prevent discharge, but it is across the obstruction, and the brass plate lever becomes much heated. The brass lever has to be pressed up against the screw to make and keep contact with some considerable force—better to have a mercury and semi-liquid surface there.

15732. But assuming the contact to be made good always by careful pressure upwds. of the lever—and the great electromagnet in the circuit: if the contact be broken by the finger so as not to

move the reflector E, the spark appears good, being bright, short and even round and starlike—but if the contact be broken by the click (15724), so that the eye mirror E moves at the time, the spark is drawn out into a line, the length apparently being 6 or 7 times the width, and at this velocity of motion of E, subtending an angle nearly equal to that subtended by the reflector G itself, it being about 0·8 of an inch wide and 3 feet from the eye. This effect is due to time, i.e. the duration of the spark thus obtained from the 5 pr. plates—the long wires and the electro magnet confer this duration. When 10 pr. of plates were employed, the spark in the still mirror E was fixed, compact and beautiful, but in the moving mirror E it was drawn out until twice as long as before.

15733. Removed the great Electro magnet (15730), retaining the 10 pr. of plates. When the mirror E was fixed, the spark was good and starlike. When mirror E moved, the spark was drawn into a line subtending as before an angle equal to that of the reflector G (15732). Must have perfectly good contacts before this point can be properly made out.

15734. Changed the reflector G for one exposing only a small bright surface about $\frac{1}{12}$ long and $\frac{1}{30}$ wide, so as to represent more fairly the reflecting needle of the galvanometer—fixed. Used 10 pr. of plates in battery but no Electromagnet. It requires careful adjustment of the reflector G to throw the ray from its small surface on to a given and constant part of the eye reflector E, especially as, because of the flatness of one surface at the contact breaking place S, the oxide and other circumstances—as the shape of the screw terminal there—the spark varies its place a little and of necessity its reflected place in E must vary also; but when right and the spark appears in E at rest, it good and bright, but confined to a particular part of the reflector E.

15735. But when the click is used so as to move mirror E, irregularities occur and the spark did not appear always—perhaps because the mirror has moved before spark appears and become out of place. By degrees found I could see the spark in the moving mirror—about the same length as before (15732), but the line weak in light though pretty uniform throughout. Still, if two object lines could be adjusted in sight at once, there was light in them to compare them.

15736. I should want to place the two simultaneous or fixed lights one over the other thus; then if the eye mirror revolved and the spark endured equally in time, they would be seen thus. Then if the galvanometer needles moved, and both alike, they would still be seen over each other and probably upright, moved as it were a little to the right or the left. But if they moved in different times, they would both have moved one way or the other, but in different degrees, as thus, the lowest having moved first and furthest. If they moved alike *equally*, *that* might be shewn by causing one to deflect to the right and the other to the left. If each spark took time, that might cause the line to be inclined a little; but still—the results could be observed and distinguished.

15737. Having this small G reflector up (15734)—put the great cylinder Electro magnet with its core into the circuit of the ten pair of plates. With the fixed reflector E, the light was much brighter than before. Also when E was moving (15727–32) could see pretty well the prolonged light. It was brighter below than above; but whether this was because the ending of the spark (in time) was brighter than the beginning (15727), or whether a better part of the mirror G was brought into sight by the motion of E, I cannot say, but believe the former was the cause.

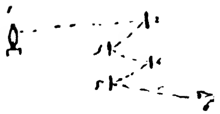
15738. By use of a little solution of cyanide of potassium and cy. mercury at place of spark S, with a little metal mercury, I rendered all the surfaces there bright and metallic. Now had an improved spark as to brightness. It would probably give a spark sensibly longer in duration—but I do not see any disadvantage in that. A perfectly momentary spark would give dots in the moving mirror E and one with a little duration, lines—but the lines could be separated as well as dots, and if separated in contrary directions might aid by their contrary inclination.

15739. The whole apparatus would require very steady arrangment and exceedingly nice adjustment, but I think it possible. The Galvanometers will have to be very steady and separate from the table on which the contact breaker is placed.

15740. Time, as a general element in transverse or Magnetic induction, is beautifully illustrated in Gassiot's New American coil—when the iron core is in. What would it be if the iron core were away? Would it appear at all? In that case it would be

an effect of true dynamic induction between the primary and secondary coils, subject to the added effect of the induction of the secondary coil one part on another.

19 MAY 1858.



15741. Diminution of the light of a candle 1 by successive reflexions from the good De la Rue metallic reflectors 2, 3, 4 and 5, to the eye at 6. There was much diminution of light by the four reflexions, but still the image of the candle very good and sufficient; by interposing a card with a small slit, made the light object small—still a good final image. By removing the candle 1 to a distance or the eye to a distance, there was no other diminution of the light than that due to mere distance along the ray. The mirrors 2, 3, 4, 5 were in this case about 3 inches apart.

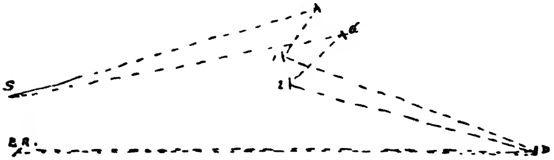
15742. The arrangement may probably be thus*. S the spark—G G the two galvanometers—1 and 2 two fixed reflectors to send the two rays on to the double and directing reflector D, by which they become double parallel rays nearly and give the two lights as one object in the moving eye reflector ER. S and ER are parts of one apparatus (   ) and should stand on a separate table from that supporting G G, 1, 2, and D, inasmuch as the motion of the click, etc. of the contact breaker (15689, 725) might communicate else a vibration to the needles of the galvanometers.

29 MAY 1858.

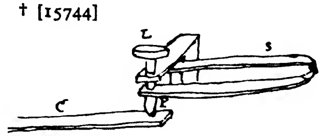
15743. With a view to perfect contact (15729), clips amalgamated at the surfaces lay hold of the battery plates. Wires soldered to them dip into mercury cups. Other wires dip into these cups and proceed to the apparatus—these are laid hold of by screws, etc. but all the bearing parts have been tinned and amalgamated, so as to have sure metallic contact both at the battery and at the contact breaker (15731). The amalgamated zinc plates and the platinum plates at the battery are considered sufficiently in contact—pieces of card having been introduced to render the wooden clips more firm in their action.

15744†. At the place of the contact breaker (15738), where the spark is to appear as a light-object between the end of the thumb screw L (15689) and the lever C as it separates from it, the thumb

* [15742]



† [15744]



screw has been replaced by a stiff spring carrying a pointed copper plug P, and the spring holds this fast up against the thumb screw, so that by the motion of the latter the spark place at the end of P is determined as the lever C separates from it. The lever and the plug are both tinned and amalgamated at the place of contact, so as to give a sure contact and a bright spark on separation.

15745. With this apparatus arranged and connected with a battery of 8 pr. of plates, the junction was well made at once, and upon letting the lever C fall, an excellent disjunctive spark was obtained at P. So it was also with only 4 pr. of plates – also with 2 pair and even with *one pair* – apparently as good as with the 8 pair, shewing that we now had good contact (15729).

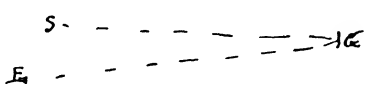
15746*. Placed this spark-place at S; arranged a fixed large De la Rue reflector $3\frac{1}{2}$ feet off at G and the eye place at E, where the observation could be made directly with the eye or in the moving or fixed eye mirror. In the fixed eye mirror the spark was very good. In the moving eye mirror also it was sharp and sudden, so that there is no longer the effect of retardation from want of contact.

15747. But the disjunctive place was soon injured, i.e. the mercury blown away and a depression made there, which by its edges hid the spark much from the mirror G. The reflector G and also the contact breaker were on separate table[s] on the stone floor. Great good resulted from this arrangement – from the altered contacts and the use of mercury at the spark place.

15748. Attached, by binding, a piece of the Electric lamp carbon on to the lever at the disjunction place. Put on the battery of 8 pair Grove's plates – had a good scintillating spark when observed directly. When looked at in the moving reflector at E there were good sparks but prolonged – the spark was lengthened. There was also imperfect contact at the binding wire at S. With four pair of plates the effect was not good – there was evidently obstruction. With 2 pair of plates the disjunctive spark was very poor.

15749. Mercury is best; returned to it at S, making a little pool of it on the lever. With 2 pair of plates the spark was very good and there was no evidence of any excessive or extra heating at the place of disjunction.

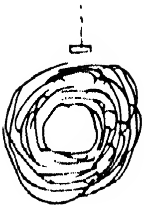
* [15746]



15750. The mirror G seems to keep its place well on the separate table.

15751. Put up the small fixed mirror at G (15734, 46). Use[d] a fixed light with a card and a hole for the first adjustment—answered very well. Soon found and arranged the spark place in E. Then put on 8 pair of plates and obtained the light image in E when moving. It was feeble, of course, being so small, but very fair; did not seem prolonged, and using an eye glass it was quite distinct. Two side by side would compare very well.

15752. Very little change of position or place would throw the reflected image out of mirror E.



15753. Took away the fixed reflector (15751) and put up a galvanometer at G, letting the needle hang above the coil but still very free to move by currents of air or any other circumstance. The shade glass was over the whole to keep off currents. Examined the needle by a light and the reflection. The needle was very steady and its face was nearly perpendicular, for when a horizontal ray was passed to it, it was reflected horizontally again.

15754. When S was a disjunctive spark, could with great difficulty only catch the reflected ray at E. Even when S was a candle, could not catch the reflected ray at E if contact were made and broken. Found that making contact deflected the needle a little and breaking contact threw it back again. Repeating thus junction and disjunction agitated the needle much and destroyed all useful results.

15755. Caught a reflected spark from disjunction of 8 pr. plates, and think it will do very well if I can separate the needle from the action of the connecting battery wires.

15756. Took two new covered wires, each about 24 feet long; twisted them so that the currents through them might neutralize each other as to external magnetic action. Connected them carefully with the battery and the contact breaker, so that the loops (inevitable) should be in planes being azimuths to the galvanometer needles. Still there was affection of the Galvanometer needle on making and breaking contact. When contact was made and the eye at E, the image in G went to the right; when contact was broken it returned to the left.

15757. Turned the whole contact breaker on the table of support clock fashion or direct, and found that a position could be obtained in which it *did not* affect the galvanometer needle; turned the contact breaker a little more and then the needle was affected in the contrary way to the former—then returned the contact breaker to its indifferent position and then the needle was not moved; returned it to its first position and then the needle was affected as at first; turned the contact breaker still more against clock motion and then the needle was still more violently affected.

15758. So the contact breaker affects the needle at this distance of $3\frac{1}{2}$ feet. There are two steel springs in it—I think it must be these—the lower and larger spring is placed obliquely to the coil that is formed by the course of the current there. By taking the upper wire end away and connecting it at once with the cup of mercury below, so as to throw out the coil formed by the parts of the contact breaker—then making and breaking contact so as to have the spark there, did not affect the Galvanometer needle; so all is at the contact breaker (15759).

JUNE 1, 1858.

15759. Have had the two iron springs taken away from the contact breaker and replaced by brass. Hope now that the needle will be unaffected by it.

15760. Have examined the Voltaic lamp light by a rapidly revolving mirror. The light is apparently *quite continuous* even when a little hissing.

29 JUNE 1858.

15761*. Placed things as before (15746)—i.e. spark or object at S—galvanometer at G—and the eye at E. Made the object a silvered glass ball about $\frac{1}{2}$ inch diameter with the gas light shining on to it; but it was utterly inefficient as an object.

15762. Battery of 5 pr. Grove's plates with the contact breaker as before (15745), using the new associated wires (15756) and the contact breaker corrected by substitution of a brass spring for the iron one (15759). Placed as object a candle flame behind a screen with a hole at S—the eye at E regarding the Galvanometer needle directly. Now no making or breaking of contact *without*

* [15761]

the battery disturbed the needle–no knocking of the table or other action. But when the battery was in connexion and the spark occurred at S, then the needle was affected as before (15754). Removing the battery 20 feet off made no difference–the effect is not in the battery or in the wires.

15763. Repeated (15757). As contact breaker stands, making contact sends the image to the right, breaking contact sends it to the left, and a few alternations gave a strong swing. Turned contact breaker watch fashion much, and then reverse effects were obtained; found a position for contact breaker in which it did *not affect* the galvanometer.

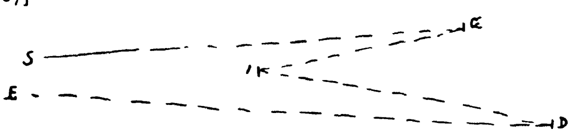
15764. With contact breaker as at first, it gave first results. Then reversed the current at the battery and now the contrary result on the needle was obtained.

15765. So effect due to the open circle which the connexions at the contact breaker form, and I must make the connexions there simpler and as close as possible to each other. As there will be two galvanometers, it will be difficult to put the contact breaker into a position equally neutral for both, unless it, the breaker, be rendered as indifferent as possible upon a galvanometer in any position.

15766. Motion of my steel spectacles upon my head sadly disturbs the galvanometer needle. I must use the tortoise shell glasses.

15767*. In reference to the required arrangement at (15742), made S a full candle flame–G the fixed reflector of De la Rue, blackened but with a minute space clear to represent the Galvanometer needle–1 a single adjusting mirror, one of Varley's pieces on the arranged microscope stand–D the directing double reflector, using one of the mirrors–and E the eye reflector on the moveable axis. Found I could adjust the whole so as to get a very fair object in E, and that when both the reflectors at D were in use, I should get the lights from the two galvanometers side by side as one object in the moving eye mirror E. Expect to do the same with the voltaic spark as object.

* [15767]



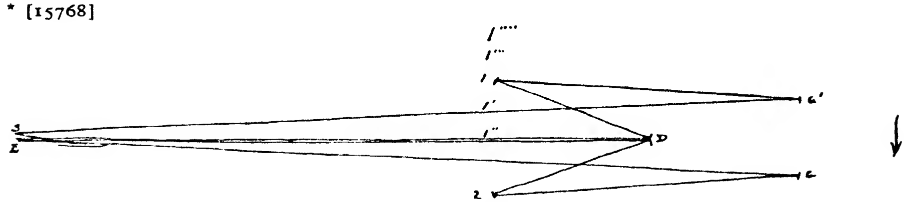
15768. Experimenting on position of the apparatus, the above* arrangement seemed very good. It gave the distance from S to G the largest between the spark apparatus S and the Galvanometer, so as to cause least affection of the latter, and the two sets of reflectors came in very well. It seemed better than the former (15742, 67).

15769. Supposing that the arrow head represented direction of magnetic north and the galvanometer needles G, G′ are in the magnetic meridian, then the rays from the spark or candle at S reflected from the Galvanometers gave the angles G1 and G2. Found that the direction of these rays was usefully under government: thus if the needle G′ was taken once round clock fashion by the action of a small magnet and then left to itself, the torsion on the suspending silk caused the needle to set round a little and the reflected ray was sent to -; a second revolution of the needle in the same direction would send the reflected ray to -. By returning the needle the ray could be sent to 1′ or 1 again or even to 1‴ and 1⁗. I think this will be available in the adjustment of the reflectors 1 and 2.

15770. The reflexion of the round glass shades is bad, coming in the wrong place. Must procure flat shades.

15771 †. If the Voltaic spark should be too long in duration, then might probably arrange a Leyden spark in conjunction with the eye mirror so as to give it at the right time. The spark ought not to be long, but dense and determinate in its place. Probably a large jar—or a well connected battery charged to a low degree—

* [15768]



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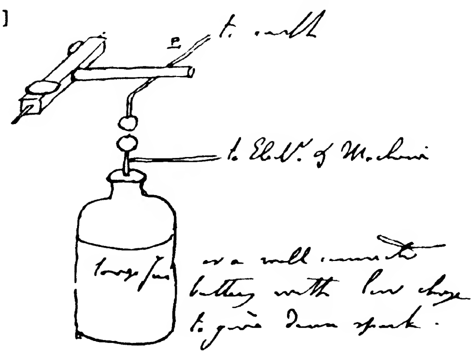
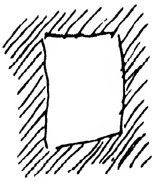


would supply such a spark. The jar might be retained charged to the proper degree by the use of a feeble discharge train acting constantly to the earth, whilst the machine was constantly acting. The lever on the mirror axis might either *depress* the earth connexion E, or might *fall away* from it, E being then a spring.

23 AUGUST 1858.



15772. Worked at the arrangement of Galvanometers, etc. Have cut holes in the sides of the shades, so that the entering and reflected ray may not be deranged and the image deformed by the action of the glass. The sheltering of the needle by the shade seems to be perfect notwithstanding these holes, which are small.

15773. Used a fixed star light at S (15768), it being the image of a small argand lamp reflected from a silvered sphere about $1\frac{1}{4}$ inch in diameter. The reflected image seen by the eye at 1 is small and feeble, and I doubt whether the Galvr. needles are good plane reflectors.

15774. The least displacement of steel spectacles, or motion of scizzars or other steel articles in the place, shewed a disturbance of the Galvanometer needle as proved by the disturbed reflected ray.

15775. I think I must be content to work with a continuous light in the first instance, and see what I can obtain with that, and afterwds. proceed to the use of a momentary light. For the fixed light, each Galvanometer may have its own light object and that had better in the first instance be near. Probably an Argand lamp with a pierced opaque chimney will do. One galvanometer image may disappear before the other if the *time* be in any case sensible.

24 AUG. 1858.

15776. Resumed the Expts. began ten years ago (9970, 9971), 24 Novr. 1848, on possible deposition of carbon slowly. The four bottles were examined and apparently had undergone no change—they were not darker in colour—no gas had been generated—no sulphurous acid—the odour was etherial as at first—the addition of a little water evolved heat—the acid was a mixture of sulphuric and sulphovinic—and the diamonds in each of the

two bottles amounted to 0·7 of a grain in weight and were unchanged in character.

30 AUG. 1858.

15777. Have been working with a constant light, and the following arrangement of parts*.

15778. A lamp or candle placed at *a* sent a ray to G′, then to 1, then to D and then to E, which was very easily adjusted for E and seen there as a bright star, that being the image of the illuminated needle of G′.

15779. When the candle or lamp was placed far back, as at S, then the ray required much more care to direct it to E, the adjustment from D to E being very nice--the image was of course feebler, and a much smaller motion of the Galvanometer needle or of the eye at E threw it out of sight.

15780. It is well at first to keep the light at S separate from the contact breaker and eye reflector; it allows of a little adjustment of the one or the other separately.

15781. It is well to raise the light at S, so that the ray going towards G should pass *over* the head and hands in the adjustment.

15782. A lime light might be used there, if desirable to increase the distance for delicacy's sake—or to use a smaller intenser light.

15783. Using a small argand lamp at S, I could get the two reflected lights from D into the eye mirror E as one object.

15784. It will be well to fix the reflectors 1, 2 and the director D together on one board or basis—allowing motion to adjust and the action of screws to clamp. That is being done.

* [15777]

15785. Surely the force of gravitation and its probable relation to other forms of force may be attacked by experiment. Let us try to think of some possibilities.

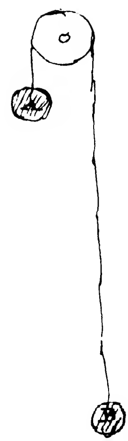
15786. Suppose a relation to exist between gravitation and electricity, and that as gravitation diminishes or increases by variation of distance, electricity either position or negative were to appear—is not likely, nevertheless try, for less likely things apparently have happened in nature.

15787. There is more chance of any observable effect in a body acted on by the earth than in the same body acted on by a like body. There is more chance of a variation being observed in a ton of water or lead when lifted a hundred yards upwards from the earth, than in the same ton when removed a hundred yards in a horizontal direction from the side of another ton by which it at first stood.

15788. Must not be deterred by the old experiments (10018, etc.). If there be any true effect of gravity, it may take much gravitating matter to make the effect sensible, and I had but very little. Moreover, the motion of a body with or against gravity ought not to form a current in a closed circuit, as tried in the former case, but perhaps give opposite states in lifted or depressed bodies, and though a current might be formed in a wire connecting two such, it would not be a current in a circuit. So may consider the imaginable effects under two views, *static* or *dynamic*. Take the former first and imagine as follows:

15789. If an insulated body, being lifted from the earth, does evolve electricity in proportion to its loss of gravitating force—then it may become charged to a very minute degree either *positive* or negative. When thus charged it may be discharged, and then if allowed to descend insulated, it would become charged in the opposite manner, and so on. If three or more bodies of the same size, but in weights as 1, 2 and 3, then the intensity of the charge ought to be as the densities.

15790. Might not two globes (or masses, as pigs) of lead be attached to the end of a long rope passing over a large pulley at



the top of the Clock tower, or in the whispering gallery of St. Paul's serve an experimental purpose. Starting with both balls insulated, discharged and balanced, then it would be easy to raise B and lower A, and examination by a very delicate static electrometer might shew A charged pos. and B negative; then discharging both and reversing the motion, B would come down Positive and A become negative and so on. The static electrometer might be applied either above or below or at both places. If the effect were real but insensible, several journeys up and down might be effected - the discharge above being made by a bell wire and touching lever. Or the discharge above and below might be made *automatically* to two electroscopes, one above and one below, so as to accumulate many results into one these electrometers being very delicate and of the condensing kind. One man, having only to turn a windlass, might work the apparatus for half a day - or it might be kept in motion by a small engine or other mechanical power.

15791 *. Perhaps a much less height would do – and then perhaps a balanced lever would do, carrying the insulated balls A and B. Place could easily be found for such, 15 or 20 feet high and about 40 feet long. A and B would then be easily manageable and weights made heavy or varied at pleasure.

15792. As to the character of the gravitating matter, it may be conducting or nonconducting – and probably an important difference might here arise. Non-conducting matter invested in conducting matter would be peculiar in its discharging action, etc.

15793. Iron – lead – platinum – copper pigs – Tin blocks – Water in vessels – Stone – Marble – Crystal.

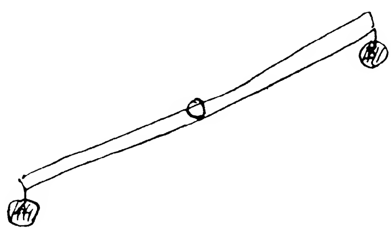
15794. It would be easy I think to distinguish between an atmospheric and a gravitation effect.

15795. The evolution of *one* electricity would be a new and very remarkable thing. The idea throws a doubt on the whole, but still try, for who knows what is possible in dealing with gravity?

15796. The first thought would give a new relation, a relation of a dual power to a single power, which would probably give a modification to the character of singleness supposed to belong to gravitation – for it would then be as dual as electricity.

15797. If the insulated masses assumed the state supposed, they

* [15791]



would when connected give a current between them and thus dynamic effects; but it is possible that if connected whilst they were changing their position, more distinct dynamic effects might be produced. A bar of metal turned end for end upwards might have a current flowing along it and especially at the middle. If in two pieces and these connected by a fine galvanometer, might have results.

15798. Perhaps a thick wire galvanometer might be better than a thin wire instrument. Insulation should be good. The oscillating bar (   ) might answer. Might construct a revolving system, connecting the ascending and descending parts by a commutator with an Electroscope or a galvanometer.

15799. Perhaps a jet of drops of water from a height would tell below–only water is a bad substance because of the discharging facility of moist air.

15800. Probably a jet of lead would do better; the fall of shot in the shot tower. Might insulate the tub of water into which it falls below and so get traces of any evolution.

15801. One of their pigs of lead, insulated and discharged below, then raised to the top and examined by the Electroscope.

15802. Two pigs of lead at opposite ends of a cord as before (15790).

15803. Might make some trial experiments at home from the Lecture room window to the floor, or even from the top of the house to the bottom of the yard. Shot might be sent from above in a stream from a metallic uninsulated vessel, a fine hopper–into an insulated vessel below. Or as lead may be liable to oxidizing action, could also use Mercury. Probably 100 lb. or 150 lb. would shew some effect and could then compare the state of one with the state of the other.

15804. Let us encourage ourselves by a little more imagination prior to experiment. Atmospheric phenomena favour the idea of the convertibility of gravitating force into Electricity, and back again probably (or perhaps then into heat). Matter is continually falling and rising in the air. The difference and the change of place of the bodies subject to Gravity would perhaps give a predominant electric state above, as the Negative; but also an occasional *charge* of the other state, the Positive. If there be this

supposed relation of gravity and Electricity, and the above space be chiefly or generally Negative—then we might expect that, as matter rises from the earth or moves against Gravity, it becomes Negative.

15805. Then we might expect a wonderful opening out of the electrical phenomena.

15806. So to say, even the changed force of Gravity as Electricity might travel above the earth's surface, changing its place and then becoming the equivalent of Gravity.

15807. Perhaps heat is the related condition of the force when change in Gravity occurs. Atmospheric phenomena are not at first sight opposed to this view. Might associate a thermo electric pile or couple, to see if change of elevation from the earth causes any sensible change of temperature.

15808. Perhaps almost all the varying phenomena of atmospheric heat, electricity, etc. may be referrible to effects of gravitation—and in that respect the latter may prove to be one of the most changeable powers instead of one of the most unchanged.

15809. Let the imagination go, guiding it by judgment and principle, but holding it in and directing it by *experiment.*

15810. If any effect, either electric or calorific—then consider the infinity of action in nature—a *planet* or a *comet* when nearer to or farther from the Sun. Dr. Winslow's observations on earthquakes—a Falling river or cascade—the falls of Niagara. Evaporation—vapour rising—rain falling—hail. Negative state of the upper regions. Condition of the inner and deeper parts of the earth—their heat. A falling stone or ærolite heated. A Volcano and the Volcanic lightning. Smoke in a chimney perhaps goes out electrified.

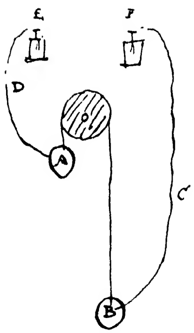
15811. What a multitude of events and changes in the atmosphere would be elucidated by such actions. I think we have been dull or blind not to have suspected some such results.

15812. If, etc.—it may be possible in the clock tower, by having a large heavy conducting mass, to obtain, after its elevation, a spark and so realise a thunder storm, in the lightning, etc. and to explode things by it.

15813. What would be the relation of the balloon experiment with the wire hanging down?

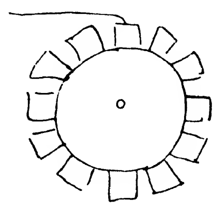
15814. If any thing results, then we should have
15815. An entirely new mode of the excitement of either heat or electy.
15816. An entirely new relation of natural forces.
15817. An analysis of Gravitation force.
15818. A justification of the conservation of force.
15819. If either heat or electricity evolved, would probably refer both them and gravitation to actions of the ether or medium in space.
15820. What relation will the Electric Zero given by the inside of a metallic vessel bear to these effects and phenomena? Will it be zero every where?
15821. The drops at a water fall may not give the state of the air at the upper part - or an effect of induction from that air upon the brim of the fall but the state related to Gravity acquired by descending.

4 MARCH 1859.



15822. A, B, heavy weights insulated. C, D, connecting wires. E, F, two electrometers—all discharged; then A down and B up—touch electrometers—B down and A up—touch the reverse electrometers, and so on for some time, using a commutator for E and F contacts. Ought to collect opposite charges.
15823. Make A a real weight and B a hollow metal vessel of the same size—then pull up and down—A should give signs—B little or none.
15824*. A fly wheel with insulated parts revolving—touch at top and bottom—should give contrary states; or wheel with insulated weights—might give a machine?
15825. What would be the electrical equivalent of a certain weight moved through a certain vertical space?
15826. That equivalent would be the equivalent of the *mechanical force* required to lift the weight through that space—also, when of the contrary sign, the equivalent of the mechanical force acquired in falling through that space.
15827. How would this relate to an equal mechanical force exerted by other means than gravity, as a push, or gunpowder? Or to exertion of force in planes across the lines of gravity force?

* [15824]



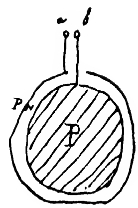
15828. Perhaps whilst the weight is falling it may not shew the electric state—only when stopped. If so, what would be the equivalent for rising—the antagonism of gravity?

15829. Motion of weights towds. or from each other in horizontal planes ought to do nothing sensible, i.e. mere motion should do nothing if not related to gravity.

15830. As to the infinitessimal difference in distance between the centre of the earth and the center of a heavy body rising or falling through 100 feet, and therefore the insensible change in the degree of the force of attraction and the impossibility of measuring it:—the gravitating force may and must vary and varies by the force required to raise the body or that produced by its fall—that force becomes the expression for the change of gravity; small as it may appear when estimated by distance effect. It is a true and probably useful expression of the change of gravity. The Electricity may detect and even measure it.

15831. What may be the Gravelectro equivalent for one ton of matter raised 100 feet high—or for any other distance or weight? Will it be the same for all kinds of matter, or is there any specific capacity or capability concerned? Will there be any difference between paramagnetic and diamagnetic bodies?

15832. Suppose a weight P surrounded by a metallic coating N P insulated from it. The weight P would evolve electricity, but the coating, because of its little weight, none. Then if P became positive, it would induce upon the inside of N P negative electricity and an equal amount of pos. on the outside. Then the ball *a* connected with an electrometer would shew a charge exactly of the nature of P. After that and when N P had been discharged external[ly], a spark ought to pass between *a* and *b* when brought together.



15833. In this way all external influence might be separated, for if P were examined for itself alone and was found to have a state, it would be independant of such influence.

15834. In shot making—the descending shower of lead ought to charge the receiving tub below, it being insulated. Another temporary tub, insulated, might be put for an experiment on the top of the usual one.

15835. Falls of Niagara—great quantity of matter falling. There

ought to be a current of discharge somewhere—probably directly into the body of the earth.

15836. *Our stairs*—free space for fall—39 feet from the stones below to the coal box platform above—and about 4 feet 10 inches more to the bottom of the hanging hook. The wheel used is a foot in diameter.

15837. Is it possible that heat, as well as or instead of electricity, may be evolved? Mr Welsh's anomaly in the temp. of the atmosphere at a given height. Also Piazzi Smyth's Teneriffe Report, Phil. Trans., 1858, page 526.

7 MARCH 1859.

15838. A Sling or loop made of thick white silk thread. Four thickness of this thread sustained half a hundred weight well when quiet, but if jerked much so as to give impetus, I could succeed in breaking the silk sometimes at the knot and sometimes else where. A sling of this silk thread consisting of 14 circles or convolutions and therefore forming a sling of 28 threads and about 9 inches long was used to suspend the horseshoe magnet. A Zamboni was employed to charge the suspended magnet by a touch, and a delicate Gold leaf electrometer (   ) was employed to test its state. The Zamboni charged the magnet and electrometer well, but in three minutes all the charge was lost by conduction through the silk. Again, the charge was nearly all lost in *one minute*. The sling is good as to strength, but as to insulation will not do.

15839. *Gutta Percha sling*. A round Gutta Percha band or rope, 0·17 of an inch thick, was made into a loop by a bad sort of tie and employed to suspend the magnet. There was an insulating space of 14 inches in length clear. The magnet being charged by the Zamboni, eighteen minutes after it was found charged, apparent[ly] as strongly as at first. Again charged and left for an hour and sixteen minutes—at the end of that time it was found still well charged. So this suspension will do as to *insulation*.

15840. Another loop made of Gutta percha, nearly 0·25 of an inch thick and having 9 inches of clear insulating length, was used with the magnet. Being electrified by the Zamboni, it kept its charge for $1\frac{1}{2}$ hours. On being recharged by the Zamboni, it

kept its charge for 4 hours and then it was very good. This good as regards insulation.

10 MARCH 1859.

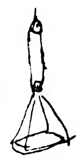
15841*. Suspended a 56 lb. weight on the thinnest Gutta Percha Sling (15839). The Electrometer was at hand and also one of the coiled wire connecters with its end support of Gutta percha in a foot, to make contact with the suspended weight. The Electrometer wire and weight being connected and the Zamboni applied to the iron suspended weight—it seemed to charge *slowly* only and not like the magnet (15843). This may be due to the increased induction on the electrometer, extended wire and on the books near to and below the weight—or perhaps to the bad conducting power of the oxide of Iron covering the weight and touched by the Zamboni.

15842. Left all charged, i.e. weight, galvanometer and the spiral connecting wire, at $10^h\ 50'$—25 minutes after, all were found fully charged—left them so and 75 minutes after that again, there was still a strong charge. So the insulation good.

15843. Filed a clean place on the iron to allow of good contact—then found the charge was quick enough (15841).

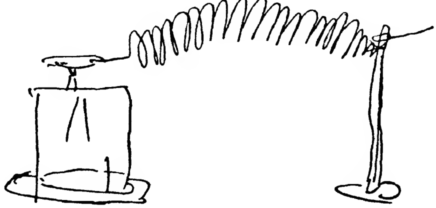
15844. Placed a piece of wet bibulous paper about 3 inches by 2 on the side of the weight, to create a damp atmosphere against it at the place. Being charged by a touch of the Zamboni, it kept a full charge for 25 minutes. Being charged again and left for an hour, found it well charged, notwithstanding the damp paper.

15845. Strength of Gutta Percha slings. A ring sling of the 0·17 gutta percha was made, with twisted and warmed joining bound round with red tape. A scale pan was hung up by it and then the length of the sling was 13·6 inches—on adding 56 lb. it stretched to 14·75 inches. 56 lb. more added after a time. Soon after, it gave way at the joint, which was very bad. Temperature 54° F.

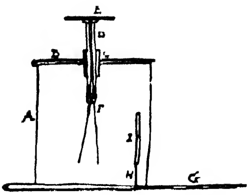


15846. Made a sling of the 0·25 gutta percha (15840): it was better in the joint than the former. With the Pan only, its length was 13 inches—put in 56 lb.—became $13\frac{1}{4}$ inches long—added a second 56 lb., length became 14·2 inches—left at $9^h\ 45'$, the parts at the joint slipping and stretching a little—was holding at $12^h\ 20'$, with signs of stretching at the joint and the length 14·75 inches—added

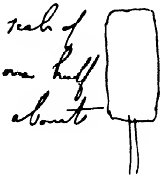
* [15841]



a third 56 lb.—at $2^h\,45'$ the length about 15 inches and I think a little further stretching at the joint—put on the fourth 56 lb. At 5 o'clk. put on the fifth 56 lb. At 7 o'clk. it was 18 inches long and had slipped or stretched or both much at the joint—the pan was touching the papers below. On taking off the 280.lb. the sling contracted in length to 16 inches—not at the joint but generally. The joint was of this form*.

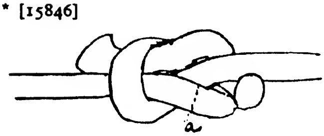


15847. Have made two electroscopes. No. 1 is of this character: A, a cylindrical glass jar—B, a brass cap to the top, having a brass tube C in the middle of it. Into this is cemented a glass tube D, which has within it a brass rod connected at the top to the brass plate E and below with the gold leaves F. This brass rod is insulated from the glass tube D by two plugs of shell lac, one under the brass cap E and the other at the bottom of the tube, just above the setting on of the gold leaves F. G is a flat brass plate carrying an upright rod H, and at the top of that a brass plate I. By slipping the glass jar along the brass plate G, the gold leaves can be brought into any degree of vicinity to I. All the brass of this instrument is kept free from varnish. The insulation and retention is so good that when the leaves are opened by a Zamboni pile to the extent of $\frac{2}{3}$ of an inch, it will keep the charge for many hours.

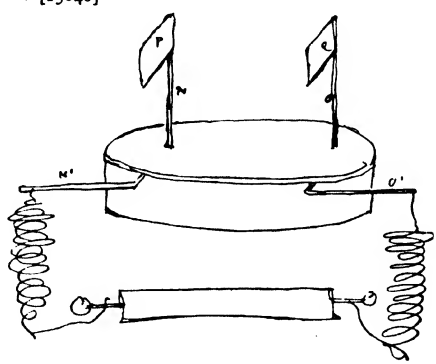


15848†. No. 2 Electroscope is, as to the jar and cap, mounted in the same way, except that the Jar is taller and there is but one gold leaf, longer than the others, about 3 inches long. The bottom is a thick cake of shell lac. Two bent stout wires N, O, are lodged in two grooves in the shell lac and are held in place by a thick sheet of Gutta Percha placed over them adhering to the shell lac. P and Q are two copper plates soldered to the tops of the wires N and O. By the motion of N′ or O′, one or both of these can be inclined inwds. or outwards so as to be nearer to or farther from the single gold leaf hanging between them. A small Zamboni column, also insulated if necessary, is connected with these two

* [15846]



† [15848]



plates so that any desired electric condition can be given to them. The gold leaf, if charged, is affected accordingly. Is not this Bohnenberger's Electrometer? Every part insulates well and holds a charge. All the metal is free from lacquer or varnish.

15849. When the leaf of gold is excited between the two charged plates or even between the uncharged plates, it often turns a little on one side. Might extend this lever principle and make a torsion instrument that, carrying a small mirror, might shew a minute deflection to a large audience, as Dubois Reymond did.

11 MARCH 1859.

15850. Arranged the Sling of thick Gutta Percha (15846) as before—put three 56 lb. weights into the pan—the length of the sling was then $17\frac{1}{2}$ inches—added two 56 lbs. more: the length became $18\frac{1}{2}$ inches—added a sixth 56 lb. weight which brought the pan on to the papers below—removed them and added a seventh 56 lb. weight. Now the pan by a little time settled on the floor and the loop was $21\frac{1}{2}$ inches long, every part having extended, but that near the tie joint most. After a few hours, shortened the rope suspenders—re-arranged the loop and pan—added the weights by degrees, and after having put in the eighth weight, the loop in about 10 minutes gave way, having supported 4 cwt. or 8 times 56 lb. The fracture was sudden and just at the knot, in a part thinned by the moulding; the place is marked in (15846) *a*. The sound unchanged part of the Gutta percha was very good.

I think such a loop of gutta percha safe for two hundred weight.

15851. The silk sling (15838) has been dipped in white hard varnish, stretched and hung up with a weight for a night—then put into a warm air cupboard for 48 hours; it is now perfectly dry. Being employed, as before, to suspend a 56 lb. weight and tested for insulation, it was found to be much better than before—it held a charge for 9 minutes—but it would not hold a zamboni charge for 47 minutes, for all sensible signs were then gone. It will not compete with gutta percha.

15852. Tried the Electrometer No. 2 (15848) with Zamboni associated, but it did not appear to me to be so delicate or advantageous in its use as No. 1 (15847).

15853. *Delicacy of indication.* Connected the 56 lb. weight, the

Electrometer and the connecting wire as in (15841), using the same smaller link of gutta percha for insulation. Made a carrier of a piece of tin foil, the size of half a crown, fixed at the end of a shell lac stem, and used the Zamboni apparatus to charge it. Six of these carrier charges conveyed to the weight gave a sensible indication at the electrometer. Good contact should always be provided.

15854. Four charges only also gave a sensible charge by this electrometer—whether the charges were from one end or the other of the Zamboni pile. Even three charges were evident by careful observation. I think that probably one may be made sensible when the earth plate is properly shortened in the electrometer.

12 MARCH 1859.

15855. At the Clock tower, Houses of Parliament. There are three shafts there—one to be a chimney for the houses—the second for the clock weight—the third is the stone staircase—it has an iron railing and brick wall and there is a square well down the middle which is 30 inches in the clear and about 200 feet in perpendicular depth. There is also a wheel and rope in it. It will be excellent for my purpose.

15856. Shot tower—near the Waterloo Bridge, Belvedere road, Lambeth, S.; belongs to Walker, Parker and Co.[1]

15857. Rearranged the broken sling of gutta percha (15850), making the knot as before (15846) but more carefully, so as not to soften or strain the part in the knot. Put it up as before with the scale pan, and then the length of the sling was $13\frac{5}{8}$ inches. Added two 56 lb.: the length became 14·75 inches. Added two more 56 lb.: the length became 16·25 inches. Temp. 56° F. In an hour the length became 17 inches—in six hours more it was 17·25 inches. In three hours more, still $17\frac{1}{4}$.

15858. Worked in the Stair case—7 o'clk. P.M. and onwds. Three gas lights alight. Employed the thick Gutta percha sling (15850, 67) to insulate a 56 lb. weight—it was well arranged and

[1] Walkers, Parker and Co. Ltd. The firm has carried on the business of lead manufacturers continuously since the date of Faraday's experiments, and the shot tower is still in daily use.

could go up and down from the floor to the level of the coal platform.

15859. Being up and connected with the Electrometer 1 (15847) by spiral wire and its stand, and then charging all with a touch of the Zamboni pile—the charge was retained pretty well for a time.

15860. Being at the bottom, the weight was raised to the top—it was discharged or uninsulated at the bottom and was found uncharged above, having received no charge in its passage through the air. This was done twice.

15861. Being at the bottom, insulated—it was charged by the Zamboni—raised and examined: found the charge in at the top. The actual lift is 38 or 39 feet (15836).

15862. Being at the top, insulated, the weight was charged by the Zamboni, being alone, and was left for 20 minutes; at the end of that time a charge was found in it but much had been lost. For on making a charge with Zamboni and immediately examining it, the effect on the Electrometer was much greater.

15863. The Electrometer and its wire, insulated but unconnected with the weight, were charged by the Zamboni. The charge was soon lost, as if the thin naked spiral wire and its fine termination discharged rather easily either to the air or the neighbouring gas burner. Must not keep the wire and electrometer connected with the object.

15864. It would seem as if the gold leaves of the Electrometer charged the air around them sometimes, for a sort of slow irregular pulsations took place in their opening and shutting—the effect was very small but still there was something.

15865. Would the effects in the day time be the same?

15866. Should the connecting fine wire be covered and varnished?

15867. The sling did not seem a very good insulator. I had wetted the knot when making it. It was of the piece of Gutta Percha broken by 4 cwt. (15850).

14 MARCH 1859.

15868. Made two long slings of the thick gutta percha of 0·25 of inch in diameter. The one was 43 inches in length when

stretched in the back room with 56 lb. Next morning at 9 o'clk.—it was still 43 inches long.

15869. The outer sling, being stretched in the stairs, was 81 inches long when the scale pan was on. Added 56 lbs., which made the length 83 inches—added 56 lb. more, which made the length 87 inches. The knot is now well set and holds fast. Left with these weights on at 7 o'clk. P.M. of the 14th March. Next morning at 9 o'clk. the length was 88$\frac{1}{2}$ nearly and the temperature 55° F. When taken down and relieved from all weight, its length was 83 inches.

15 MARCH.

15870. Insulation of the long loops. The 56 lb. up in my book room by the loop (15868) 43 inches long. Charged by Zamboni. After 30′ it was found fully charged. Charged again by Zamboni—after 4^h 40′ all charge apparently gone. Charged at 10^h 25′ P.M.—next morning all charge gone.

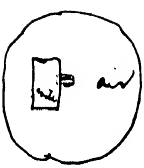
15871. The discharge may be along the Gutta Percha or across the air, etc. by dust and convection, etc. When the Electrometer II (15848) and its insulated wire were near the weight, charging the weight made the gold leaf move towards its opposed discharging plate. This was by an effect of induction through the air, the charged weight being inductric to the wire and Electrometer cap.

15872. One end of Zamboni was uninsulated. A fine communicating wire attached to the other end had its free end brought within 1$\frac{1}{2}$ inches of the suspended weight. After 1^h 20′ the weight, examined, did not appear to have received any charge.

15873. A block of copper weighs 112 lbs.

A Pig of lead weighs 1$\frac{1}{2}$ cwt. or 168 lbs.

15874. On the idea that heat may be evolved or absorbed as gravity is effectively exerted or opposed, it probably would not be right to associate a thermo-electric pile with a mass of metal and air, because the whole pile being metal would charge as the metal would. The pile must be kept below or above in an unexceptionable state and connected with the metal under trial after rising or falling.



15875. If any charge, it ought not to be greater for a large mass

than for a small one, so that only mass enough to affect the thermo electric battery fairly will be required in the trial experiments.

16 MARCH 1859.

15876. Arrangement of 56 lb. and sling of 43 inches as before (15870). The bottom of the weight was about an inch from a stool beneath it, and the spiral communicating wires were placed near the side to see if there would be discharge to them and so quick loss of charge. The weight charged by the Zamboni *Pos.* at $10^h\ 7'$—at $12^h\ 30'$ or after $2^h\ 23'$ the weight was still fairly charged though it had lost part of the Electricity.

15877. Then stool and wires away from the neighbourhood, after which it kept a powerful charge for $45'$—and also for $3^h\ 5'$. Being left charged at $4^h\ 20'$, it was found all discharged or gone at $10^h\ 20'$, i.e. after six hours. This long sling insulates very well.

17 MARCH 1859.

15878. Put up in my book room the long gutta Percha sling (15869) with two convolution[s], i.e. four strands—it was consequently 41 inches long—would bear 4, 5 or 6 Cwt. well (15846), but I want to know its insulating power under these circumstances—a half hundred weight was attached to it.

15879. The weight was charged Pos. by Zamboni at $10^h\ 7'$, being unconnected with the Electrometer; $18'$ afterwds. it was found well charged. It was charged Pos. again at $10^h\ 25'$—after $4^h\ 15'$ there was still considerable charge in the weight. It was charged again at $2^h\ 40'$. Six hours afterwds. the charge was gone. It was recharged at $8^h\ 40'$. Two hours after, it was only poorly charged. The weight was all this time exposed to the air, dust, fibres, etc. in the corner of the library. The insulation seems sufficient.

15880. The weight was charged Pos. and then a fine wire connected with the Electrometer held within half an inch of its side for ten minutes, but the wire did not draw off any sensible charge to the Galvanometer in that time.

15881. The oxide of iron on the side of the iron weight and also the oxide which forms on a cleaned copper wire after a day or

two offer much obstruction to the communication of electricity of these low intensities.

18 MARCH 1859.

15882. Charged the weight (15878) Pos. at 7^h 40′–after 1^h 40′ found it well charged and took out four successive charges by touching the electrometer cap before the remaining electricity became insensible. Charged it again Pos. at 9^h 20′–$4\frac{1}{2}$ hours after, there was a little charge remaining. At 1^h 50′ charged it Neg. by the Zamboni–$2\frac{1}{2}$ hours after, the weight was still charged and fairly–the insulation will do.

19 MARCH 1859.

15883. The electrometer A, No. 1 (15847) has had the cap covered with platinum to give good contact–also made stiff in the stem–the lower part of the glass has been covered with paper to prevent excitement by touching. Being alone and charged Neg. by the Zamboni at 5^h 35′, it was well diverged 20′ after–after an hour it retained half divergence–and at the end of the $1\frac{1}{2}$ hours there was still divergence.

15884. Being charged Positive by Zamboni at 7^h 5′, it went down much by 8 o'clk. Being recharged Pos. at 8^h 0, it went down a half or more in 12′. I expect some small spider's web, or else the Pos. does not hold so well as the Negative.

15885. If the glass jar becomes excited by accidental touching, it is easily discharged by breathing upon the places, and this does no harm to the instrument.

21 MARCH 1859.

15886. Examined the thermoelectric pile and its multiplier. There is no difficulty in connecting the pile and the multiplier by wires without producing a sensible current. The instrument did not seem to be sensible enough for my purpose, since a momentary touch of the finger did little. However, a slight puff of the mouth was very sensible and sent the needle far off.

15887. The 56 lb. weight–a short gutta percha sling about 10 inches long–annealed connecting wires with platinum ends–copper of the weight cleaned–all connected and charged at

$10^h\ 30'$–was not high–at $11^h\ 40'$ all charge gone. Suspect floating films or fibres. So

15888. Dusted round the weight and instrument–reconnected all together and charged Neg. at $11^h\ 40'$–was very well charged at $11^h\ 53'$–also at $12^h\ 4'$–also at $12^h\ 15'$. Even at 1 o'clk. there was considerable charge. When the gold leaves were brought near the platinum earth plate, the charge was very manifest, and by the same means a trace was discovered as late as $5^h\ 30'$, or six hours after the first charge.

15889. The wiping (15884, 8) is evidently good. Must probably sweep the shaft of any place for experiment by a frame, and cloths either dry or damp fastened to it, in order to carry off all spider's films, etc.

15890. At $5^h\ 40'$ the weight alone was charged Negative by Zamboni–at 8.45 connected with the Electrometer and found charged–at 10.15 all charge gone.

22 MAR. 1859.

15891. The weight, sling, Electrometer, etc. as before (15887). Passed a silk handkerchief round them all and over the face of the near books, walls, etc. to remove the spiders' films, etc. in the air; connected all together. Charged the whole Pos. by Zamboni at $9^h\ 25'$. The opening of the gold leaves was excellent and the handkerchief had evidently done good. I think the operation will be necessary generally. At 12 o'clk. all was well charged. At 3 o'clk. still well charged. At $4^h\ 15'$ there was a fair opening of the leaves. At $7^h\ 45'$ the leaves were nearly collapsed, but by approaching the earth platinum plate there was fair evidence of a remaining charge. This was above ten hours after the first charge. At $10^h\ 40'$ P.M. all signs of the charge were absent.

15892. I have had a condenser made, of two circular plates of brass about $\frac{1}{12}$ of an inch thick and 2·8 inches diameter. The surface *a* of the lower plate is platinum, soldered all round at the edge. The upper surface *b* of the same plate and the lower surface *c* of the upper plate are ground and covered each with two coats of Shell lac dissolved in absolute alcohol. Being well dried, these form two insulating dielectric plates through which the induction of the condenser is to go on. The upper surface *d* of the upper

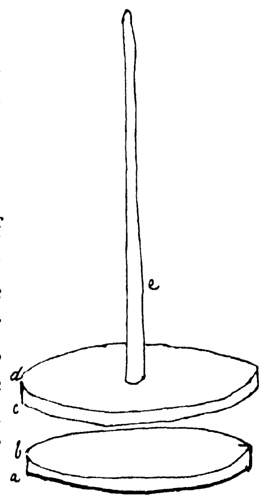
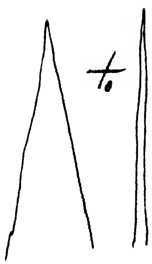


plate is unvarnished and of metallic brass. A brass handle is screwed into this face of the plate. When the surface *a*, being platinum, is put on to the platinum top of the Electrometer, immediate and certain contact is obtained, and when the upper plate is placed on the lower and any charge given to the electrometer, induction goes on through the two coats of shell lac towards the upper plate.

23 MARCH 1859.



15893. Connected the weight and Electrometer by the spiral wire as before (15887)—placed the lower condenser plate on the Electrometer cap—then charged the whole Pos. by Zamboni at $9^h\ 33'$ and then put on the upper condenser plate uninsulated. This reduced the divergence from [see figure]. Left the upper plate on, insulated. At 9.50 raised the upper condenser plate—the divergence opened out to the first degree—there seemed to have been no loss by conduction across the shell lac layers on the plates. Restored the upper condenser plate and left it insulated.

15894. At $10^h\ 37'$ the charge was still excellent. At $1^h\ 40'$ full half the charge remained. At 5 o'clk., when the upper condenser plate was raised, there was divergence of the leaves and the charge in them, etc. was rendered abundantly evident by bringing the earth platinum plate near them. At 9h. P.M. there was sensible divergence when the upper plate was removed. When the gold leaves were first brought near the earth platinum plate and *then* the upper condenser plate was removed, there was divergence and striking discharge to the earth plate, and then the discharge of all was complete.

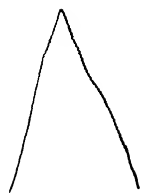
15895. Thus a part of this charge had been held for twelve hours nearly, without discharge either through the air—by films—by the sling—or by the shell lac of the condenser.

15896. This evening at 9 o'clk. all was connected and charged Pos. as before (15893), the upper plate of the condenser being in place but insulated. Next morning all charge was gone, and I expect from filaments or dust particles rather than from conduction across the shell lac.

15897. All connected as yesterday (15887, 93). If the upper condenser plate be away and a charge be given to the weight, Electrometer, etc., then putting on the upper plate causes induction and lowers the divergence: if the upper plate be raised, the full divergence appears—if it be lowered, it ceases; and if then the upper and lower be uninsulated, all charge disappears, for on then raising the upper plate there is no divergence. So the plates of shell lac had not been penetrated and had received no charge.

15898. Or if the upper plate be first put into its place—then a charge given to the weight, etc. by the Zamboni, and *then* the upper plate be touched, the previous great divergence of course falls and then things go on just as described (15897).

15899. On the other hand: if the upper plate be first put into place—then uninsulated and then the charge given to the weight, it requires time to supply the requisite electricity to cause full divergence, and of course there is far stronger induction at the condenser. If now after 2 or 3 seconds only the top and bottom plates be touched and then insulated, all signs of divergence disappear, but on raising the upper plate a large divergence comes on. Putting on the top plate, this ceases; but raising it again, the divergence reappears. This divergence is evidently due to a charge on the shell lac of the lower plate, which remains on the electrometer. When this effect is produced, the upper plate may be placed on the lower and both uninsulated for five seconds or more, and yet, being then insulated and separated, the high divergence is produced; but if while the two plates are separated they are *uninsulated*, then on bringing them together all charge appears to be gone. I do not conclude therefore that it is the surface of shell lac against the metal of the upper and lower plates that has been charged by penetration of electricity, but rather think it is the outer surface of the two, which being separated by a thin film of air whilst in face of each other, became charged from the air by disturbance when the direction of the induction was changed, as happened in the Reiss and other cases.



15900. The short slight gutta percha sling which has now been up with the 56 lb. weight for three days (15887) is now $10\frac{1}{4}$ inches long. So there is no sign of it giving way.

15901. Went to the Shot tower. Mr Walker not there but examined the tower. There is a stage or flooring midway for making small shot—it is about 90 feet from the floor to this stage and 185 feet from the floor to the upper stage or floor. There is a chain up the middle of the tower and down the outside to the engine, and there is a box in this chain to carry up the lead—the chain is always there, going through large trap doors. There are two apertures besides in the middle floor, one for a fall of fine shot from this floor to the tub below, and the other for a fall of larger shot from the upper tower; the clear fall in the first case is about 90 feet and in the second about 185 feet: these holes are perhaps 18 inches square. Besides the chain there is a rope, to pull up a bag of shot, which runs over a pulley and may perhaps hold 10 lb.—this would do for the heat apparatus. But I should want a crab to lift a cwt. weight.

15902. Saturday afternoons are leisure—as the men do not then work.

15903. Mr K. O. Hodgson gave me a letter of introduction to Mr Walker.

15904. Pigs of lead weigh differently—some are $1\frac{3}{4}$ cwt.—others $1\frac{1}{2}$ cwt.—and some 1 cwt. I can have any one for an experiment.

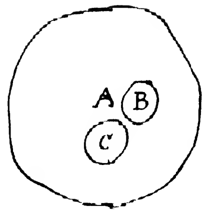
28 MAR. 1859.

15905. Sling of the thicker Gutta percha of 0·25 diameter—long, so that when up in my book room in three coils and with 56 lb. on it, it was 3 feet long. It and the Electrometer were connected and charged Pos. by Zamboni at 11 o'clk. It remained pretty well charged at $1^h\ 20'$—and even at 3 o'clk. fair charge was found when the earth platinum was brought near the gold leaf. Being recharged at 3 o'clk., it was found charged a little at 6.30—so that the six strands will do very well and give great strength.

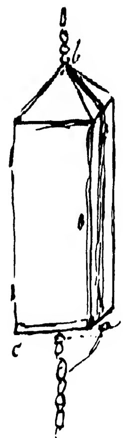
15906. On Tuesday 29th March, this sling was up below stairs and, with the scale pan and one 56 lb., was 37 inches long—with three 56 lb. it was 38 inches long—with $5\frac{1}{2}$ it was 39 inches long. At 10 o'clk. A.M. the charge was *Nine* 56 lbs. weight and length $41\frac{1}{2}$ inches. At 10.30 P.M. the pan just touched the bundle of papers beneath. On Wednesday at 11 o'clk. it had sunk a little lower and was $41\frac{1}{2}$ inches long. At 12 o'clk. the weight was taken

out, except one 56 lb.—it contracted in length immediately to $40\frac{1}{2}$ inches. Being left then until Saturday morng., it was then $39\frac{3}{4}$ inches long and was the same in the eveng. Quite strong enough for Four 56 lb.

11 APRIL 1859.

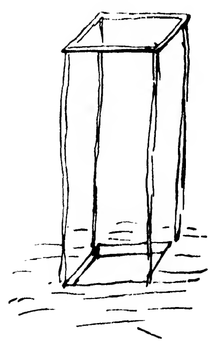


15907. Went to the Shot tower again to obtain further data. On the basement floor there are three places concerning me: the place of the lead box, which is in the middle of the floor or nearly so, and the places of the two tubs which correspond to the shot falls from the middle and upper floor. A is the place of the lead box, and B and C are the places of the tubs.



15908. The lead box is of strong wood, 36 inches high by $12 \times 12\frac{1}{2}$ inches wide. A strong iron band goes round it from the top to the bottom and up again. This at the top is fastened to a chain, which goes up through the middle of the tower, then over pullies in the inside of the roof, through the side over a pully and down the outside of the tower to the bottom, where it can be coiled by the steam engine on to a drum when lead is drawn up. A chain hangs from the bottom of the box which, elongating as the box rises, keeps it partly in equipoise, but 2 pigs of lead are always kept in the box to bring it down when empty. Now these chains are *fixed* to the box; but when I want to raise a pig insulated, it will be easy to hold the lower chain to one side, as at *a*, by a chain from it to the top of the box at *b* and then by another chain going from thence to *c*, to make a good insulating suspension for a pig of lead there.

15909. I could also easily arrange a shade over the ascending pig, so as to carry away cobwebs.



15910. The first or middle floor is 84 feet from the ground. Here the casting hole belonging to the furnace is 12 inches wide in the floor and has an iron frame about it, raised 4 feet from the ground or floor, to support the square cullender or sieve. But there is another hole in the floor which corresponds to the casting floor above, and this is larger and more convenient for me. It is an opening 14×18 inches, and it is surrounded by a boxing, perhaps 3 feet high and 24×30 inches wide. The top of this frame

will support a wheel very well for me for the thermometer experiments (15973, 4).

15911. The upper floor is 165 feet from the ground. Its casting hole is like that below and has a like iron frame which can carry a wheel; the thickness and strength of the upper iron rim which supports the sieve is 1·25 inches by 0·4 of an inch and so fully strong for me.

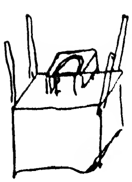
15912. There is a cord at the tower to raise a sample of shot. It is strong enough to raise a few lbs. and it has been so long in use that I think all twist has been taken out of it—it is over the middle hole or that for the box, which has doors. I think I should like to have sash line.

15913. If the temperature of bodies should be affected, then their capacity for heat should affect the amount of charge.

15914. The following point is against electricity. There is no reason why, when two bodies recede or approach, that they should not change (if they change at all) in the same direction. But this would be against the idea of a dual power—though not against that of a single power (so to speak), as heat.

15915. It would be strange if a body should heat as gravitation increases by nearness of distance. We conceive of heat as a positive force and of gravitation as a positive force, and then instead of being the inverse of each other, they would seem to grow up together. Or else heat must be negative to gravity or the converse of gravity and gravity must be in the same negative or converse relation to heat. This is against the expectation of any thing from the heat experiment. Nevertheless make it, for who knows. If gravitation depend upon forces *external* to the particles, such results might happen. Try.

15916. Have constructed two apparatus for trials of any heat effects. One is a square box containing two differential chambers. Each of these contains an air thermometer bulb. One is to be surrounded by mercury, the other by air; and when all is arranged, they are to be connected externally by a scale and fluid tube, so as to form a differential thermometer, and this shew any change in the temperature of the chambers. The outer box is of Mahogany—9 inches long—6 broad—and 7 high. Within it are



two cylindrical chambers each inches high and inches in diameter within. The glass bulbs within are inch in diameter. Their stems are continued to the outside and are there to be connected by the scale part. The trials with this apparatus are given, together with the dates, in the following notes (15918–).

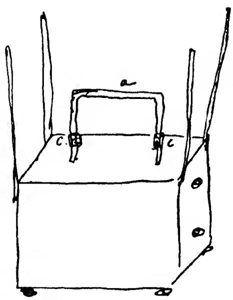
15917. Another instrument was constructed, first by Mr Ladd and then a second by Mr Caselli[1]. This was just a large delicate Mercurial thermometer, containing about a pound of mercury. It and its trials, with dates, are generally described in the following notes (15940–).

15917 *a*. *May* 21. Expd. in the Laboratory with Dr. B. Jones' two Malaptereri[2]. Dr. B. Jones, Mr Beckett, Tyndall. Shock good. By passing electric current either of the Leyden jar or of the Ruhmkorf through the water, the fish shewed their thorough sense of the electricity and appeared to have no defence against it, whether they were across the current or were head or tail to it. By using temporary saddles of platinum plates, we obtained decomposition of the iodide of potassium, and the place of decomposition shewed that the current was from the posterior end of the fish through the wires (and water) to the anterior end—the reverse of the Gymnotus. In one case evolution of iodine appeared at both platinum electrodes, shewing that either a spark had passed there or that the fish could give currents in both directions, which is not very probable.

Tuesday, 12 April 1859.

The differential air thermometer apparatus.

15918*. The scale part *a* is given full size below[3]—a few portions of coloured spirit have been introduced into it in broken columns—and then attached by two pieces of vulcanized rubber, *c*, *c*, to the

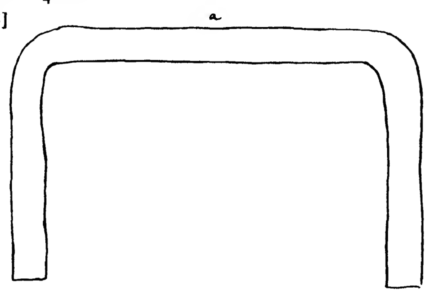


[1] For Casella. Faraday's spelling of the name varied: see pars. 15937 and 15957 *et seq.*

[2] Malapterurus. A genus of catfishes, certain species of which have the property of giving an electric shock when handled. *O.E.D.*

[3] Reduced to $\frac{3}{4}$ scale.

* [15918]



air thermometers within the box. They are tied by thread and there is a little coloured spirit in one of the bulbs in the box. Left at 6^h 45′ P.M.

15919. On Wednesday morning, the 13th, a change in the places of the spirit columns had occurred—no doubt because of the settling equalization of the temperature of the bulbs in the box. Marked the position at 7^h 30′ A.M.—the temp. outside being 57° F. Perhaps the same at 11^h o′ A.M. At 9^h o′ P.M. the columns had moved to the right about $\frac{1}{8}$ of an inch. On Thursday morning, 14th, at 8.45 A.M. the columns were in the same place. The same at 5^h 15′ P.M. Also on the morning of Friday, 15th, the places were the same. So when the temperature within the box is equalized—then the variations of temp. outside, which were at least from 56° to 61°, caused no change of place.

15920. On inclining the box so as to let gravity act on the columns, they moved freely—so no sticking. Pressure or motion at the Vulcan rubber connexions caused much change of place either one way or the other, as desired. They should be stiff—tight and have as little air capacity as possible—also be dry and not be disturbed during observations or experiments.

15921. The extreme columns or cylinders of fluid, when very small, shew by their further reduction that they lose a little in bulk. The change does not seem to be between them and the intermediate cylinders, which preserve their bulk, but seems to be due to a slight distillation from the spirit in them to the interior of the air thermometers during the hotter portions of the day.

15922. The piece *a*, the scale or measuring piece, was taken off, cleared of fluid and recharged with broken cylinders. The air thermometers and box was inverted, that fluid might drain out—the vulcan rubber connections made dry—and then all put together as before with care and the position marked at 5.45 P.M.; though the bulbs had not been exposed to change of temperature yet the stems and connections had. Left all night until 7^h 30′ A.M. on morng. of Saturday, 16th. A given column had then moved about an inch to the left—due to the change of temperature necessar[il]y occurring in the two stems.

15923. Tried the effect of external circumstances. Jogging or vibration does not change the place of the fluid. Inclination does because gravitation of the columns comes into play. For this reason there should be no fluid in the vertical parts of the stems—nor at or in the caoutchouc connexions. Perhaps a tube of this form* would be good, especially with a stay or so at *c*, *c*.

16 APRIL.

15924. Filled both chambers with Mercury—attached an ivory scale to the tube. Left the apparatus on its side and the scale tube horizontal—as in the figure†. The fluid was very steady except when connexion 2 was pressed. At $5^h\ 0'$ P.M. the fluid was as in the figure, *a* being 50° and *b* 74° on the attached scale. Change of position of the box and scale makes a change of place. On restoring position, the fluid does not go accurately back. Also sometimes whilst handling it the fluid sets off with a jerk, as if there were capillary obstruction somewhere. The fluid was left between 18° and 43° at $5^h\ 25'$.

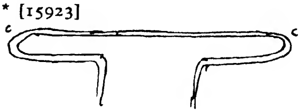
At $6^h\ 40'$ the columns were thus‡—and think now that temperatures are equalized within.

15925. 17*th April*, at $8^h\ 1'$ A.M.—same position—inclining the scale makes the fluid move slowly either way—but if left displaced, it does not move back again at once on restoring the box position. At $9^h\ 40'$ P.M. it had returned to its $6^h\ 40'$ position.

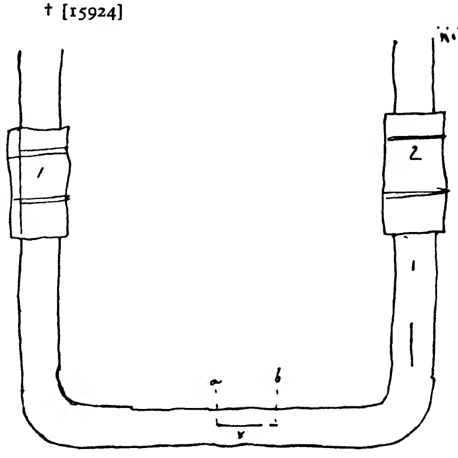
15926. 18*th April*, $8^h\ 45'$ A.M. No change in place of fluid—it has not evaporated—by change of position of the tube the fluid changes place easily and on restoration of position goes back slowly to its place.

15927. 3 *May*. Faults in the above apparatus. The connexions

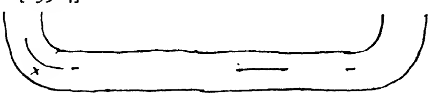
* [15923]



† [15924]

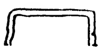


‡ [15924]

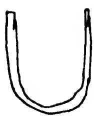


1 and 2 are not air tight, though bound by thread very tightly or even by *wire*: the application of white hard varnish at angles made them tight, and then any fluid, as *a b*, being displaced by Gravity, would return on restoration of the first position. Varnish however does not adhere to vulcanized rubber, but easily rubs off. Gum is the same, but not so easily.

15928. The chambers filled with mercury on the 16 April. One marked 1 has run and let the mercury out. Found that it ran at a certain spot in the bottom worm marked ′, and not at the insertion of the thermometers. Not any where else. Marked the place for Mr Ladd's attention. Bibulous paper washer does not cure it.



15929. The scale part *a* (see note, 12 April) is a source of great irregularity; because of the bends, the capacity and form of the space for fluid varies, and consequently when a broken column of coloured alcohol and air is in the tube, a portion of the fluid is drawn into this angle and is held there by the counterbalanced capillary forces developed at the two end surfaces of the portion. This is able, when one surface is in the narrowest part of the tube, to sustain an inch of the fluid in a vertical position against its gravity, and draws the broken column into position against the elastic force of the air in one or the other bulb. The tube had better be curved thus and have a sensibly equal diameter in all its parts. Perhaps also it ought to be made so as to retain its position in a horizontal plane, to get rid of changes from gravity.

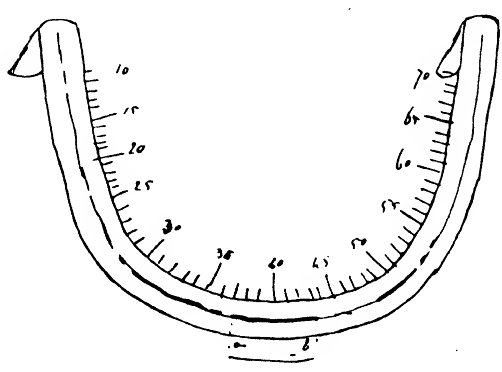


16 MAY 1859.

15930*. A piece of scale tube of this size[1] and curvature—connected as on 16 April by Vulcan rubber pieces tied with wire and varnished. Portions of red spirit fluid being in the scale, an irregular paper scale was put under the tube and the portion

[1] Reduced to ¾ scale.

* [15930]



of fluid *a b* selected as the indicating part. The position of this tube was *horizontal* in the scale part, so as to be free from the effect of gravity on the position of the fluid column. Every thing appeared right and tight. When the tube was lifted on one side or the other the column gravitated, but on restoring the position of the tube the fluid resumed its place, shewing all was tight.

15931. 17 May 1859. At 10 A.M. *a b* was at 37°·5–45°. At 4 P.M. it was at 36°–43°·5. Difference due to little variation of temperature on two sides of box standing on the table near the open door. Put mercury into both cells. Then fluid at 39°·8–47°·1.

20 MAY 1859.

15932. Mr Ladd has put washers to the chambers, and they were filled with mercury on the 16th. Since then they have been shaken about, but seem all quite tight. To-day emptied the mercury out of No. 1 chamber–closed it up and packed the box full of well dried and sifted mahogany sawdust. All was done well–the indicating fluid is of the same form as that depicted for the 16th (15930). By inclination of the box it moves freely in the tube. If the box be returned at once, the fluid returns to its place at once. If the box be inclined till fluid vertical, it descends much and does not quite return to its place if the fluid remain vertical a little while. I expect that the connecting pieces leak a very little and that the white hard varnish over them has become quite dry and cracked; must try a tough varnish.

15 33. 23 *May*–the indicating fluid still keeps its form and place.

26 MAY.

15934. Tried this apparatus to-day with the arranged pulley and the selected sash line in our stair case, to see how it would work at the shot tower. It was slung by a loop 2 feet long from the top to the eyes in the box side, that it might at all times keep its level. The weight of the charged apparatus is 10 lbs. and all worked well, but the following result startled me.

15935. Marking one particular termination of the indicating fluid–when the box was at the top it was found to stand at 42° of my irregular scale–but when lowered to the bottom the fluid had

gone back to 40°. When raised again it slowly went from 40 to 41°·5. Thinking this might be due to the difference in level of the platform above and the floor beneath, or to a difference in the position of the box when standing on either and when suspended, it was observed both above and below when suspended and when no change in its level could have taken place. Being above, it was at 41°·5—being below, it went to 40°. I did not wait to see if it would go lower, but raising it, the fluid again advanced and went to 41° and by degrees to 41°·5. The temperature below was lower than the temperature above, but I do not see how that could cause the change. Nevertheless, I will cloathe all the parts of the stems.

15936. I can hardly think it possible that this is a Gravity effect, for the height is only 36 feet: but it is encouraging, for the philosopher must hope against hope or he will do nothing great.

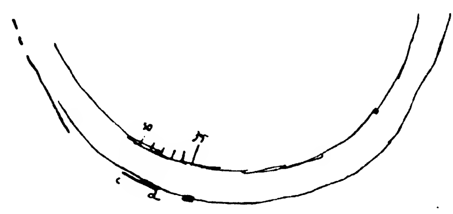
15937. Now the fluid passes from cell 1 to cell 2 in descending cell 1 contains air—cell 2 mercury (15967). So the appearance is as if the air heated in descending, or as if the mercury cooled. The compression of the air by change of altitude is a possible cause. Whether it be the air or the Mercury which changes temperature will be shewn me by the Casselli instrument.

27 MAY 1859.

15938*. Applied Copal Varnish and Alcohol over the rubber connectors and joints. In the eveng., when dried, put flannel round the stems up to the circular horizontal part in several thicknesses—then left it to settle in temperature and place. A few hours later, the fluid in the stem had travelled toward cell 2, and had probably partly entered the connecting pieces, for the disposition is different. Take *c d* as the indicating column. Probably the varnishing and the vapour of the Alcohol has had something to do with it.

15939. 28 *May* 1859. This morning the fluid as above figured (15938)*. A little inclination sends it either way, and all seems clear. It also returns well to its place on replacement of position. Slung it as before in our stair-case—using the coal pulley (15934), at about 9 o'clk. A.M. and proceeded to observe in succession above and below. See (15966). I used the termination *d* as the indicator.

* [15938]



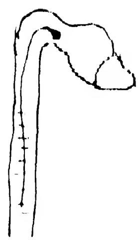
15940. The great mercury ball thermometer in its cup, stand, etc.—trial of its habits—is on my table near the door—has a scale on the glass rising from 0° to 16°, each degree being ½ an inch in length and divided into tenths.

At 4^h 40′ it was at 14°·86—at 6^h 20′ it had sunk to 10°·88, so that it changes freely.

15941. Next day, the 13th, I covered it with a glass jar, large, and included a common thermometer, so as to shew generally the temperature and proportion of size in the graduation.

At 7^h 45 A.M.	the instrument	was	3°·75	and the	thermometer	57°
10^h 0	”	”	2°·64	”	”	56°
11^h 0	”	”	3°·49	”	”	56°·8
1^h 40	”	”		”	”	60°

but now the mercury in the instrument was above the scale and swelling into the upper vacant chamber. There was a little break in the mercury at 13°·75 and this went on rising fast because of the increasing temperature: at 5^h 30 P.M. mercury in the chamber neck, i.e. break was there, the thermometer being now 61°·5. At 5^h 45′—broke the mercury away in the neck, leaving less for the graduation, but the termination is still in the bend. At 9^h 1 P.M. found the temperature falling and the mercury, which had receded, was as low as 7°·8, thermometer being 58°·5. This is probably a good place for mercury termination.



15942. Supposing that the instrument and the thermometer changed together above, then a degree in the instrument would be nearly a degree Fahrenheit, but it would be half an inch in length. Was left at night as follows:

9^h 0 P.M. . . 7°·8 . . 58°·5

Next day, the 14th, examd.:

7^h 40′ A.M. . . 0°·54 . . 56°

This would give above 3° on the instrument for one on the thermometer or 1° F.—and as the night has passed, this is probably nearer right than the former estimate.

7^h 40′ A.M.	Instrument	$0°·54$	thermometer	56° F.	
10.1	„	„ 0·54	„	56°	
12.0	„	„ 0·54	„	56°	
	Fire made in the room about 12 o'clk.				
2^h 45′ P.M.		$11°·10$		$60°·2$	
4 „		above bend		$61°·7$	
5.15 „		down a little		60·8	
6.0 „		$15°·00$		60·8	– so instrument slower than thermometer.
11.0 „		up in neck again		61·3	

MORNING. 15 APRIL.

7^h 45′ A.M.	down to	0·9	57°	
9 „	still at	0·9	58°	instrument slower than T.
10.0 „		5·35	59°	

15943. Supposing that 6 o'clk. yesterday eveng. and 7^h 45′ o'clk. this morng. gave like temperatures for the thermometer and the instrument, then each degree of Fahrenheit would be represented by 3·7 degrees on the scale, i.e. by 1·85 inches in length.

Now rearranged the Jar with another and *more sensible mercurial thermometer*, but instrument in the same state.

10^h 20′	instrument at	$7°·85$	thermr.	62° F.	
11.0	„ „	8·45	„	$60°·3$	inst. rising – thermr. falling.
1.0 P.M.	„ „	$6°·50$	„	60°	
4.0 „	„ „	in the neck	„	63·8	} too high in temp.
6.0 „	„ „	in the neck	„	64	} too high in temp.
8.15 „	„ „	$16°·5$	„	$61°·2$	
11 „	„ „	12·45	„	61°	– inconsistent.
11.25 „	„ „	11·14	„	60°	

Here a fall of thermometer of only 0·2 of a degree at 8^h 15′ o'clk. accompanies a descent in the instrument of above 4° – and then immediately after, a fall of a whole thermometer degree causes only $1°·31$ fall in the instrument.

There is no accordance here, but things were left as they were for the night.

15 APRIL 1859[1].

15944. This morng. went to the table at 7^h 30′ A.M. – found the instrument at 2° 35 and the thermometer at 56°. Whilst I

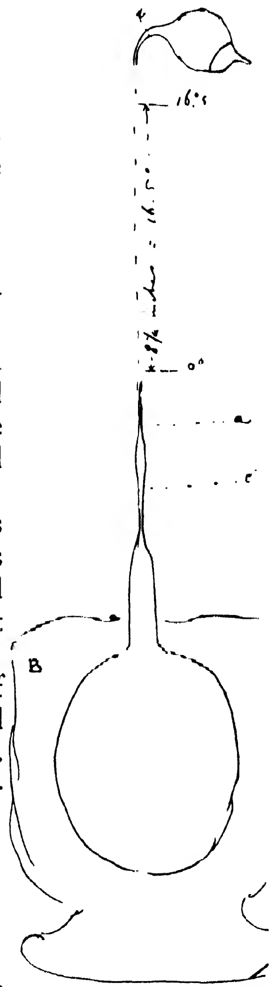
[1] ? 16 April 1859.

looked at the instrument, [it] fell half an inch or a full degree, i.e. to 1°·3. On tapping on the table it fell again to 0·8 and then tapping and thumping on the table caused no change. So it falls intermittingly—requires a vibration—and then acquires its true place. The true register therefore is:

7.30 A.M.	0·8	56° F.
at 9.20 „	0.8 still	58·2

Perceive there is a break in the Mercury, and that has probably caused the fall effect.

15945. The apparent size of the instrument cavity and its form at top and bottom is about as represented[1], B being the turned box which holds it. Up to *a* must be considered as part of the capacity of the instrument and wants cloathing—the break referred to above is at *c*.



15946. Opened the box and examined the instrument. There is a little air in the inside. The mercury runs well as the tube is inverted, but the bubble which appears in the ball when inverted contracts to a very small space when the whole is upright but a lens shews it. It is better in the ball than in the stem.

15947. 16 April. 3^h 0 P.M. Thermometer 62°·7. Mercury of instrument in the bend (*d*)—increased the temperature and lessened the mercury at *d*. At 4^h 10′ mercury still in bend and therm. 65°·2. At 5^h 30′ P.M. lessened the mercury—left it in the bend—thermometer 66°·1 F.

At 6^h 30 P.M.	12·85	63°
7.40 „	9·4	64°
9.40 „	13·6	64°·5
10.40 „	12·82	64°·2

The temperature of the instrument had been raised and not settled at these times.

17th April

7^h 50′ A.M.	quite below scale	45°·0 F.
9 40 P.M.	Do.	60°

18th April

7^h 45′ A.M.	quite below the scale	54°·0

[1] Reduced to $\frac{3}{4}$ scale.

15949[1]. Have packed the instrument in flannel and fitted it into the deal box. At night the mercury was up in the bend at *d*, but as flannel, etc. have been handled and warmed, I expect the night will bring the mercury into the scale. The chief conduction of heat will be by glass stem—may clothe that or inclose it by a glass tube.

4TH MAY.

9 o'clk.	In the instrument, at	4°·9	outside of box, thermtr. 58° F.
12 „	„ „	6·3	„ „ 61° F.

Other results of the day shewed that the outer thermometer indicated quickest.

15950. *May 5th.* At 4^h 20—temp. 67° F. Brought the body of mercury together above and by a given shake broke it off, leaving the stem mercury in the neck.

P.M. 4^h 20′ temp.	67° F.	Mercury in the neck	the difference in temperature is 6°·4 F.—the difference in the instrument is from 3°·3 to the top of the scale (i.e.) and over into the neck, i.e. 13°·5 and all over.
7^h 0 „	68°	Do.	
10^h 0 „	65·2	Do.	

May 6th.

7.50 A.M.	60·6	3·3	
	64	13·44	
	64·8	15·14	Again the instrument travels after the thermometer.
	64·8	15·7	
	65	16·6	
2.0 P.M.	67·5	in the bend a little	
7.0 „	68	Do. . . much	

At 67° F. Mercury broken off above; at 63° and at 64°·8 it was at the top of the scale, and at 60·6 it was at the bottom. So that if it be required to occupy the middle of the scale at any given temperature, it should be broken off in the neck about 5° F. above that temperature.

May 7th.

7.40 A.M.	63°·0	15·1
8.40 „	63°·5	14·25
12.20 „	69°·0	in the neck
2.30 P.M.	73°	mercury shook off in bulb
9 „	69°	„ still in the neck
10.0 „	68·2	Do. . . but less

May 8th.

8.0 A.M.	66·3	13·1	
8.45 „	66·5	12·3	yet same temperature—shew how slow to acquire new temperature.
10.15 P.M.	66·5	17·5	
10.50 „	66·6	17	

[1] There is no par. no. 15948 in the MS.

May 9th.

7.30 A.M.	64·5	4·5	
10.0 „	64·6	1·9	instrument sinking whilst thermometer rising.
11.0 „	65	2·12	now instrument rising also.

15951. Took the instrument naked into the laboratory and experimented with it. Being placed with stem horizontal–Fahr. 60°–mercury at 0° of its scale–the finger was applied to the ball–the mercury went up the scale by expansion, not regularly but in short starts–if the finger is applied intermittingly the starts can be made to correspond with it. There is some cause of irregularity existing within. On cooling, the same effect occurs, the starts being from ½ a degree to 1½ degrees. Once there was a click sound occurring in a large retraction. When the stem was vertical there was a like irregular result. Whilst expanding, the mercury surface was always convex but it varied a little in degree. When descending, it was always concave, and upon a descent it still was concave, shewing a continual sticking to the glass. Tapping with wood at the top, end on, broke up these intermitting motions but did not prevent them.

15952. When the temperature was falling and the instrument stem horizontal, raising it to the vertical position and so putting on a pressure of mercury made it sink suddenly; and on taking off this pressure by putting the stem horizontal, it rose again, but not to full extent, because the temperature was sinking. The following are cases:

H	V.	H
60	54	57
50	43	45
45	37	39·5
39	33	34·5
34	29	31
28	22	23
23	18·5	19
18	12·5	13
10·5	5	5½

H	fall	V	rise	H
165	15 [*sic*]	148	10	158
154	16	138	9	147
145	15	130	9	139
139	14	125	9	134
134	13	121	8	129
129	12	117	8	125
125	13	112	8	120
120	12	108	7	115
115	12	103	7	110
107	13	94	6	100
100	10	90	5·5	95·5
95·5	11·5	84	6	90

So here we see the fall and the succeeding rise again consequent upon the change of pressure and the diminution of the effect as the column diminishes.

15953. Made some expts. with the globe in a bath to prevent change of temperature. 154 went down to 141—change of 13°; and 146 to 137·5—change of 8°. In better experiments the changes were:

150		or	122	116	109	30
			8	8	7	2
162			130	124	116	28
			9			
150			121			
			7			
162	The average change being 12°		128	105		25
				7		1
149				112		26
161						
149						

Being of course large changes for high column and small for low column.

15954. Worked to know how many degrees on the instrument equalled a degree of Fahrenheit, so put the instrument and a thermometer into a bath and kept agitating the fluid with a stirrer. At 65° F. the instrument was at 115°—at 64° F. it was at 76° and 68°, so 1° F. equals about 47° on the instrument. Again, 65°·8 F.=155° and 62°·8 F.=4; so 3° F.=151° on the instrument, or 1° F. =50°. Again, 66° F.=160 and 64° F.=3, the instrument being vertical; here 2° F.=157° on the instrument. Some source of error within.

15955. The hand suddenly applied to this instrument shewed the retraction of the mercury due to the sudden expansion of the glass very well at the first instant.

15956. The instrument seems very imperfect and badly made. There is the appearance of a crack round the upper end near the extreme. Furthermore, the retreating mercury has left particles sticking in various parts of the tube above, shewing the irregularity and bad state there. On the 12th May, on examining it again, I found in the lower part of the stem a small column of clear fluid, either alcohol or water there. On warming the mercury it rose and was enough to wet the inside of the scale nearly to the top. The instrument will not do.

I have ordered another of Casella. 11th May 1859.

15957. 19 *May* 1859. The Casella instrument–about same size as the former (see 15 April)–a little larger perhaps. It contains nearly a pound of mercury. The scale is $7\frac{1}{2}$ inches long; every inch is divided into twenty parts, every ten parts is called and figured as a degree on an arbitrary scale, so that the whole scale has 15 or rather 150°. These degrees may be divided by the eye into $\frac{1}{5}$ or even closer. A degree of Fahrenheit is equal to about 47 of these arbitrary degrees, or is $2\frac{3}{8}$ inches in length. When the temperature is about 40° F. the mercury is at the top of the scale. The bore is flat–the mercury apparently very bright and clear.

15958. 20 *May*. Examined the instrument. Mercury ascends and descends very nearly regular, i.e. there is very little starting or jumping–tapping on the top will cure that.

15959. When resting on the bulb in a horizontal position, and then raised with stem into vertical position, the mercury sinks in the stem because of the pressure of the vertical column. When laid down, it moves back again more or less. With a short column reaching only to the 24°, the successive positions were as follows–24–21·5–25–23–25·7–23·6–26–24; temperature was rising slowly–difference is about 2°.

15960. When the column was longer, reaching to 129°, then the difference was greater. The series of numbers were thus: 128·9–123·8–130·6–125·6–131·5–126·4–133–128–133·8–128·5, etc. the difference being about 5° or 6°.

15961. When the instrument was horizontal, and then raised into the vertical position, the bulb resting all the time on paper under water, the column took a given position–being then raised by the stem so as to let the bulb hang, the mercury fell about 1°. This was the proportion when the height was only to 14° or 20°–and it was the same in amount when the height was great, being at 130°. Neither did tapping at the end prior to lifting up cause any difference. The effect appears to be due to an expansion of the ball of Glass when it has to sustain the mercury within it. If a bladder were filled with water, the same thing would happen gros[s]ly. As long as the resting state of the instrument is *unchanged*, the effect does not occur and will not interfere.

15962. I think the little jumping or starting as the mercury ascend[s] or descends is due to this power of variation in the ball–

this slight range to the set of the particles and consequent irregularity in the variation of the size of the ball and size of the included mercury. But tapping on the top seems to set me free from this if the position of the instrument be not changed.

15963. As to the required difference of temperature between breaking off the mercury above and a given position of the mercury in the stem: the instrument, with a thermometer, was put into a bath and the temp. raised to 76° F.—the mercury was then in contact with the store in the upper bulb. It was broken off—then on cooling the bath to 74° F. the mercury went to 98° in the instrument, and at 72° F. went to 0°. The following are further successive observations.

F.		inst.
72° F.	=	0°
72°·3	=	7°
74°·6	=	111°·5
71°·9	=	5° below 0°
72°·7		30° plus
75°	=	136°·5

So 2° F. suffice to bring it from breaking off to about ⅔rds up the scale. From 72° to 74°·6 F., i.e. 2·6 degrees F., the degrees on the instrument are 111·5, or 43 for each degree of F. From 71°·9 to 75° F., or 3·1 degrees, the degrees on the instrument are 141·5, or 45 for each F.

15964. Brought the mercury in contact in the bulb above, and then broke off at 64°·8 F. On lowering temperature the mercury fell in the instrument to 112° at 62°·5 F. and to minus 3° at 60°·2 F. Here, 2·3 degrees F. brought mercury from breaking point to 112 in the scale, which is quite near enough for me, and the number of degrees for each degree of Faht. is about 47. Also near enough to give assurance, for the observations were not very carefully made.

15965. 20 *May* 1859. Mr Siemens mentioned to me his Gravimeter instrument—being an attempt to measure heights by the difference of Gravity. I have not seen the instrument, but think it is some form of a bulb and mercury—but do not know. Nor do I know whether he has obtained any indications.

15966. Differential instrument from 15939.

Observations 9–10 o'clk. A.M.–temp. about 64° or 65°. Difference in altitude is 36 feet.

Above		*Below*
32°·5		
	0·1	
		32·6
	0·0	
32·6		
	0·0	
		32·6
	0·0	
32·6		
	0·0	
		32·6
	0·2	
32·8		
	0·2	
		33

So there is no change due to difference of altitude only a little due to gain in temperature perhaps, if the cells be right, but I must examine them.

There is an effect due to striæ in the glass, so that the place of the fluid appears half a degree different according as it is looked at from the convex or concave side of the scale. In fact, the degree lines are not perpendicular to the stem but inclined, so that the observation must always be made from the same point of sight.

30 MAY 1859.

15967. Opened the differential apparatus–found Cell ii full of Mercury and cell i full of air (15937)–all was right within–no mercury has got out (15937).

28 MAY 1859.

Casella instrument, from (15964).

15968. On the 26th. Thus far the bulb has been cloathed in thick flannel and that again put into a glass Jar. To-day, took it out of the flannel and packed it in the middle of the jar, surrounding it by dry Mahogany saw dust (   ). There is about   inches in thickness all round the bulb. Put flannel at the top in the jar

in many folds. Put flannel round the jar and placed it in the box. Left it to settle.

15969. On the 27th the position of things in the morng. was as follows:

	9^h 0′ A.M.	65° F. . .	instrument at	32
	11^h 30 „	66°·2 . .	„ „	60
	2.15 P.M.	68°·0 . .	„ „	105
	5.0 „	70°·0 . .	„ „	in the bulb above
	11^h 0 „	68° . .	„ „	Do.
28th.	8.0 A.M.	66°·4 . .	„ „	100·4

So the conduction is such that the instrument changes slowly, following the thermometer.

15970. This morning, 28th, this instrument (Casella) was experimented with just as the differential box app. had been used. I observed above and Anderson below, and it is probably that there is a difference in the personal equation. The results were thus:

Above		*Below*
96		
	0·2	
		96·2
	0·2	
96·4		
	0·1	
		96·5
	0·5	
97		
	0·0	
		97
	0·4	
97·4		
	0·1	
		97·5
	0·5	
98		
	0·3	
		98·3
	0·7	
99		

So there is a gradual increase of temperature due to handling, breathing, etc. But the increase appears to intermit, for the average for descending is 0·14 whilst that for ascending is 0·46, or three times as much. Perhaps Anderson and I observe

differently. Must try that by experiment. The difference in altitude 36 feet.

30 MAY 1859.

15971. Have had an eye piece fitted to the *Casella* instrument: it consists simply of a single small lens, about $1\frac{1}{2}$ inch focus, in a holder griping the stem of the instrument and able to move and be fixed any w[h]ere upon it. It is easy to divide the degrees into tenths by the eye, but much care has to be taken regarding parallax in relation to the graduation on the front of the stem and the mercury in the middle of the stem.

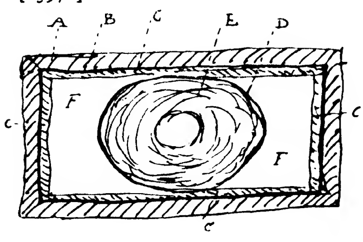
31 MAY 1859.

15972*. Packed up the Casella thus. A is the plan of the deal box, the outer case: it is 6×4 inches within. B is a square mill board box fitting it and 4 inches deep. C, C, C, C, are four sides of dry cork about $\frac{1}{8}$ to $\frac{1}{6}$ of an inch thick. D is a cylindrical paper jar (cartridge paper) $4\frac{1}{2}$ inches high, with paper bottom a single thickness of flannel goes under the bottom between it and the mill board case B and also up the front and back between [it] and the cork plates there. E is rolled flannel carefully enclosing and packing the bulb of the instrument so that the latter is more than an inch (perhaps $1\frac{1}{2}$ inch) from the paper box D. A compound cake of flannels also comes down on the top when the whole is arranged in the box. F, F, is well packed up with well dried fine Mahogany saw dust. The whole is now in the box and is just left to settle—the box thermometer being at 67°·2 and the Casella at 97° (15975).

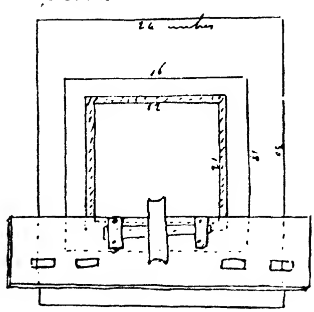
15973. The differential apparatus weighs 10 lb. The Casella instrument and box weighs $5\frac{1}{4}$ lb. The sash line rope is 228 feet long when stretched with a fair pull and weighs not 2 lbs.

15974†. Dimensions of the holes in the floors of the tower (15910, 1) and form of pulley which I have provided.

* [15972]



† [15974]



15975. The Casella instrument as described (15972) has been observed on several days as follows:

		F.	Inst.
T. 31 May	10h 0 A.M.	67°·2	97°
	12.0	69·0	86°
	2.0 P.M.	69·4	96°·4
	4.30	70·2	160
	6.0	69·7	160
W. 1 June	8.20 A.M.	68	75·2
	9	68	74·8
	10	67·8	74·7
	11	67·8	74·6
	1 P.M.	68	74
	3.10	69	83·5
	4.45	69·5	113
	11.30	68	120
Th. 2nd June	7.45 A.M.	67	32
	9.25 P.M.	70·2	in bulb
F. 3 June	9.0 A.M.	69·3	132·5

The hottest time for the thermometer seems to be about 4 o'clk. P.M. The instrument is behind the thermometer and shews its greatest expansion long after that hour; its rising going on often after the thermometer has begun to fall.

3 JUNE 1859.

15976. This day Anderson and I made successive observations on the instrument as it, being stationary in a corner but suspended, shewed the increasing action of heat. The thermometer in the box was about 71° F.; the time from 11h 8′ to 11h 40′ A.M.

1 . .	141·2		21 . .	145·6	
	141·2	. . 2		145·6	. . 22
3 . .	141·6		23 . .	146	
	141·7	. . 4		146	. . 24
5 . .	141·8		25 . .	146·2	
	141·9	. . 6		146·4	. . 26
7 . .	141·8		27 . .	146·5	
	142	. . 8		146·9	. . 28
9 . .	142·4		29 . .	146·8	
	142·4	. . 10		147	. . 30
11 . .	142·5		31 . .	147	
	142·4	. . 12		147·1	. . 32
13 . .	143		33 . .	147·3	
	143	. . 14		147·6	. . 34
15 . .	143·1		35 . .	147·5	
	143	. . 16		147·7	. . 36
17 . .	143		37 . .	147·9	
	143	. . 18		148	. . 38
19 . .	145				
	145	. . 20			

Anderson made his observations of fractions of a degree in $\frac{1}{8}$, $\frac{1}{2}$, $\frac{1}{4}$, $\frac{3}{8}$, etc. etc., but in the column I have reduced them to the nearest decimal, that they might compare with mine. The temperature was rising all the time, but evidently quicker at the end than the beginning, because of the hands, breath, etc., but only from 141·2 to 148, which is not more than $\frac{1}{6}$ of a degree of Fahrenheit in the half hour.

It is evident that Anderson and I agree very well in our reading—he read immediately after me, and then after him there was waiting for a little time. We used our spectacles but not the lens on the stem.

4TH JUNE 1859.

15977. Went to the Shot tower with Casella instrument (15972). Men all away had Anderson and Chapman. Placed Anderson below to observe whilst Chapman and I went to the first floor. Placed the wheel (15974), which answers very well. The line also answers, being the braided sash line. The plan of operation [was] for me to observe first above, then let down the Instrument, when Anderson observed below—then I drew it up, when we observed it above—then let it down and so on. The following columns contain our successive observations, those above in black ink, those below in red[1].

Self above	Anderson
	147°
152°	
	out
158°	

The tower and the weather were very hot—and the mercury was soon above the scale. So took out the instrument—warm[ed] it by the hand—ejected mercury above and then cooled it by water until the mercury was near the bottom of the graduation, 35° or about. Replaced all and began to observe again.

15978.

	Self	Anderson	
	44°·2		
		55°	. . 10·8
6 . .	61°		

[1] The columns headed "Anderson," and the right hand columns of differences, in pars. 15977–15979, are in red in the MS.

	[6 . .	61°]		
			67 . .	6
	7 . .	74°		
			79 . .	5
78° F.	4·5 . .	83°·5		
			88·4 . .	5 [*sic*]
	6 . .	94°·5		
			97 . .	2·5
	5 . .	102°		
			105·4 . .	3·4
	5·8 . .	111°·2		
			117 . .	5·8
	7 . .	124°		
			126·4 . .	2·4
	5·1 . .	131°·5		
			135 . .	3·5
	5·2 . .	140°·2		
			143 . .	2·8
79° F.	5 . .	148°		
			neck	4·3 average
Average	5·8			

Here the instrument rose rapidly and no wonder for a first observation. The temperatures of 78° and 79° F. were those of a little thermometer in the box of the apparatus, but the Casella had probably been cooled below the temperature of the box and packing and so rose quicker than need be. Now it went on rising, but the average of rise between my observations and Anderson's, i.e. between the upper and lower, is less than between the lower and upper, being in the first case 4°·3 and in the second 5°·8. What this difference is due to has to be discovered. I think on the whole that the time would be shortest between my observation and Anderson, and that would give less rise.

15979. Now went to the upper floor or top of the tower and made two or three observations which gave very nearly equal ascents.

		95°		
			106° . .	11
	9·5 . .	115°·5		
			126 . .	10·5
83°·5 F.	11·8 . .	137°·8		

But as the mercury was near the top of the scale, I threw off some and brought the surface of the mercury down as before.

Then the numbers were as follows, Anderson observing as before below.

		Self	Anderson	
84° F.		4°		
			19°	15
	5	24		
			29	5
	6	35		
			40	5
	7	47		
			52	5
	6	58		
			62	4
	6	68		
			73	5
	6	79		
			83	4
	4	87		
			93	6
	4·2	97·2		
			100	2·8
	4	104		
			107	3
	7	114		
			116	2
	5	121		
			123	2
	4	127		
			129	2
	2	131		
			132	1
	3	135		
			136	1
82° F.	3·2	139·2		
			139	0
Average	4·83	Above	Average	3·4

It will be seen that there is a difference in the same direction as before and nearly the same in amount. This looks as [if] the increase in height had done nothing.

15980. After the last recorded observation below, when the instrument came up the mercury was above the scale and there was I think more in the bulb than ought to be. This must be the result of an accident, and it is to be noted that whilst Chapman drew it up it struck at the edge of the aperture in the lower floor—it

had done so once or twice before. I suspect the column of mercury has been thrown up by the blow, or that the lower bulb is cracked. Came home bringing the Casella with me. In the evening it was still up above the graduation. Next morning, it had sunk down and was out of sight below.

6 JUNE 1859.

15981. Found the instrument had not been injured (   )—the mercury had been bumped up. Restored contact and broke of[f] in the bulb after 2½ hours, thermometer being 72° F.

11 JUNE 1859.

15982. At the shot tower again. No fire in upper floor to-day—all advantageous. Placed Anderson below Chapman on the first floor to guard the *Casella* apparatus through the floor and was myself above. The following were the observations—in order—the state of the mercury was favourable, beginning at 18·5 and rising up to the top, during which 34 observations were made.

	Above		Below	
72° F.	18·5			
		4	22·5	
		3·5		
	26			
		3·5	29·5	
		2·7		
	32·2			
		2·8	35	
		2·5		
	37·5			
		3·5	41	
		3		
75° F.	44			
		4	48	
		3·4		
	51·4			
		6·6	58	glove handed here and so time lengthened
		3		
76° F.	61			
		4	65	
		4		
	69			
		3	72	

	[Above		Below
			72]
		3·7	
	75·7		
		4·3	80
		3·4	
	83·4		
		3·6	87
		4·3	
	91·3		
		3·7	95
		4	
	99		
		3·5	102·5
		5	
	107·5		
		5 [*sic*]	112
		5·5	
	117·5		
		3·5	121
		4·5 [*sic*]	
	126·5		
		3·5	130
		5	
	135		
		4	139
		5·5	
77°·6 F.	144·5		
		——	over the top.

These observations shew a gradual rise of temperature, but nothing to indicate an effect due to gravity, for if the 16 differences descending from me to Anderson be taken, they give an average of 3·91 due to the gradual accession of temperature, and if the 16 differences ascending from Anderson to me be taken, they give an average of 3·92 due to the accession—and though the times may not have been accurately equal, still the results do not indicate any heat effect due to gravity.

15983. Now I replaced the Casella apparatus by the differential apparatus ( ), which is not affected [by] *general* change of temperature but only by a difference coming on between the two bulbs. In this case, the indicating surface chosen ( ) stood at 34° on my arbitrary scale. In the beginning, Anderson observed below—Chapman observed as the apparatus passed the first

floor—I observed at the top—then as the apparatus descended Chapman observed at the second floor—and Anderson at the bottom—and again as the apparatus ascended Chapman observed again as it ascended. So Chapman observed twice for our once, and assuming equal intervals of time to be employed between the ascent and descent, and equal intervals between the first and second floor, the results may stand as follows—although time indeed does not come into account but only space.

15984.

Andn.	Chapn.	Faraday
34	—	—
—	34·2	—
—	—	34·2
—	33	—
33	—	—
—	32·8	—
—	—	31·5
—	31·3	—
31·2	—	—
—	31·2	—
—	—	31·2
—	31	—
31·2	—	—
—	31	—
—	—	31
—	30·8	—
31	—	—
—	30·4	—
—	—	30·5
—	30·1	—
30	—	—
—	29·9	—
—	—	30
—	29·7	—
29·7	—	—

Now here there is a difference, but it is not between above and below; but all the observations shew that the fluid gradually went to one side just as if that side became of a lower temperature during the experiment. The difference was from 34 to 29·7, or 4°·3. When

the instrument was taken home and observed again at 7 o'clk. P.M. the fluid had *returned* to 34·5. The temperature of the tower was warmer than that of the instruments (15982) and we know by the former observations that the box and its contents would be slowly receiving heat during the experiment. Now the access of heat would be pretty nearly equal to the two cells, but as Cell I is the air cell, and cell II is mercury (15967), and as the fluid whilst going back on the scale would be going towards cell II, so it would seem that cell II did not warm so fast as cell I, because of the differences in the masses of matter, and this I believe is the cause of the change at the tower and of the return to 34·5 when the temperatures were equalized.

15985. So there is no evidence by either apparatus that any difference due to gravity varying by an elevation of 165 feet (15911) can shew a relation to heat by causing a change of temperature.

9 JULY 1859. Shot tower.

15986. At the shot tower, to try for Electricity. The lead box (   ) has had the counterbalance chain beneath it removed and is charged within with lead pigs, so as to go up and down properly when associated by the break with the Steam Engine. The break is one acting by friction, so that in the event of a pull on it above, the friction allows for the ceasing motion without causing any destruction of continuity. The ascent and descent of the box is under perfect command.

15987. A pig of lead of about 170 lbs. had an iron pin and shackle put through one end. On to this was attached one of my prepared hooks with a wooden block to receive (   ) the Gutta percha sling. A like hook with wooden block was hung on to the bottom of the lead box, and the large sling of Gutta percha was between the two hooks, for the suspension and insulation of the lead. This sling, which is of Gutta percha 0·25 in diameter, when made as now into a coil of three convolutions, is 3 feet long and contains 6 strands. Now such a single strand has carried in trial 2 Cwt., and I think therefore that the safe strength at these present temperatures may be estimated at 1 Cwt. per strand. Other slings and strengths are referred to 15846–50, 69, 78, 905, 6.

15988. The Electroscopes (   ) have platinum tops, and to make their contact with the lead, I had steel spikes prepared with each a piece of platinum wire brazed on to it. One of these spikes was driven into the lead, and the contact of the platinum cap of the electrometer made with the platinum attached to the spike.

15989. As to the insulation of the lead pig—when suspended in the air. It was charged by a little friction with a piece of Gutta Percha and ten minutes after gave a very good charge still. It was then discharged by a single touch. Being again charged by contact with the Zamboni pile (   ), ten minutes after it gave a charge scarcely sensibly diminished.

15990. The pig was charged by Zamboni—then sent to the top of the tower, 165 feet, then lowered and examined; it still gave a fair charge of electricity though the charge had in some degree diminished.

15991. Now sent the pig to the top—there Chapman touched and uninsulated it—then it was lowered and examined for electricity; but there were no signs of any charge. Repeated the experiment and with exactly the same negative result.

15992. Now charged the lead by Zamboni—sent it up—did *not* touch or uninsulate it—brought it down and again examined it. It was very fairly charged, shewing that there was little or no loss in the transit up and down.

15993. I now employed a fine delicate condenser on the cap of the electrometer to increase any signs of Electricity. Manipulating with it without any charge in the lead, there were no indications produced by the use or action of the condenser alone.

15994. The lead was raised—uninsulated above—lowered—examined by the Electrometer and condenser; there were signs, and the charge brought down *was Positive.* Repeated the experiment—first ascertaining that the lead discharged gave no results with the condenser—and then raising the lead—uninsulating it—lowering and examining it; when it again gave very slight doubtful results.

15995. Tried a third time—no results.

15996. Put on a second pig of 1 Cwt. of lead, so as to make the whole weight of metal about 280 lbs. Still there were no clear signs of any effect.

15997. The experiments were well made but the results are negative. Must try if contact of single pr. of plates will affect this electrometer and condenser.

15998. Mr Wickens is superintendent at the lead works. Mr Graham is the Engineer.

15999. *Ice and wool—regelation.* Thomson's paper in R.S. proceedings, x, 152.

Some lightly spun fine wool for fancy work was soaked and boiled in distilled water to make it thoroughly wet, and was then left to cool. All air was excluded from it in this way and a thread of it sank slowly in water, being slightly heavier than it.

16000. Some pieces of ice were put into a jar of water. Some of the woollen threads were laid on the floating ice, and also a little ball of the wetted ice [? wool]. After an hour, the wool had not frozen to the ice. The whole surface of the ice which floated in the water was in a thawing state, being rounded, polished and wet. The jar was wrapped up in flannel, but of course heat was entering it both through the water and by the air.

16001. The whole was left until next morning. Much of the ice melted—and a depression formed under the ball of wool, shewing that it had conducted heat from the air to the ice and melted the latter. Yet at the bottom of the hole the wool was frozen to the ice by a few threads, and I cannot conceive that any act of pressure could do this, resulting from the pressure of the wet woollen fibre and its water. Also, of the single thread of wool lying on the ice, it was frozen in places to the ice.

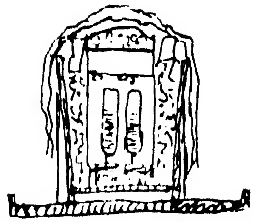
1 FEBY. 1860.

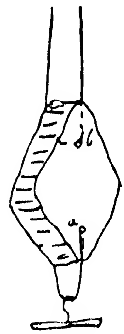
16002. Pulverized the ice—put it with ice cold water into a glass jar wrapped in flannel—the floating ice was about 2 inches deep and it was easy to arrange the surface of it, by a glass or wooden rod, so as to have a depression or pit the bottom of which was under water and where the water was pretty sure to be ice cold. Gently laid the wet woollen thread across and in this place as well as between the ice elsewhere, but with no force except its own slight weight in water. After two hours, raised the woollen thread—it drew up several small pieces of ice which had frozen to it. Restored the wool and left it until the evening. Part of the ice had disappeared by general thawing, but more ice had frozen to the wool than in the morng.

16003. The wool was replaced on the ice last night. This morng. the whole quantity of ice was less—but the quantity frozen to the wool was increased. Restored and left until the eveng. Now the ice left unthawed was much reduced, but the quantity adhering to the wool in different parts, more than ever.

4 FEBY. 1860.

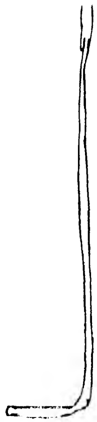
16004. A cylindrical glass jar, 10 inches deep and 7 inches wide, was placed on a three footed iron trivet within a pail—which pail was put into a circular band box for an outside jacket, and the whole placed on a wooden tray—the space between the pail and the glass jar was filled in well with broken ice. Distilled water was put into the jar 5 inches deep, and clean Wenham lake ice in lumps and pieces added to bring all to 32° F. A glass dish larger than the aperture of the jar formed a cover to it and was filled with broken ice, to keep the air in the jar at 32. An inverted plate covered the dish and four thicknesses of flannel covered the whole. Two slabs of clear ice, about $2\frac{1}{2}$ inches square and 0·8 of an inch thick, had holes made through them by hot wires at *a* and *b*—a loop of the wet woollen thread (15999) was passed through *a* and hooked on to a leaden weight by a wire hook which, when all was in the water, served to keep the ice under water and in the position indicated. A piece of the like woollen thread, passed through the hole *b*, served to lower the ice and weight into the ice-cold water and could be withdrawn when the pieces were in place. Before being put into the water, one face of each was flattened on a warm iron plate and then on a glass plate; and a groove was sunk between the hole *a* and the lower angle of the ice, that the holding loop of woollen thread might not hold the surfaces apart.





16005. The pieces were now adjusted in the ice-cold water (the greater lumps of cooling ice being removed), so that they stood with their flattened faces toward each other and about half an inch apart. When either one was moved by a splinter of wood towards the other, it immediately moved back, separating from the other when the wooden splinter was removed. So that the tendency of the pieces when together was to *separate.*

16006. When the faces of the two pieces were brought together



and held for a moment with the slightest touch, they clung to each other (being frozen together) with just sufficient force to hold them, and indeed the slightest force of separation separated them. But being brought together by such a slight touch, and then covered up and left at 12h. 45—they were found still clinging together at 1 o'clk., and again at 4 o'clk.—and again at 6.30 o'clk. At that time they clung together with considerable force. I made two copper wire lifters, inserted these into the holes *b* of the two pieces, took them out of the water as *one piece* of ice—they resisted the pulls of the wire and weights, and it was only [when] the mass *dropped* from the wires on to some broken ice placed to receive it that the two pieces separated. There had evidently been much additional freezing near the places of contact, though the mechanical forces in action tended not to pressure but to separation and extension.

16007. The pieces were replaced in the water at 7 P.M. with a few pieces of clean ice floating over them—and the very gentle contact having been renewed enough to cause adhesion—the whole was left for some hours.

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16008. The bath in good condition at 12 o'clk. The pieces, sticking well together, are perhaps about half their first size—shews gradual melting in the very middle. The pieces do not seem to freeze more together by long standing. Being put together at first as slightly as possible, they seem to increase in adhesion for a moment and then continue adhering. The two weights were removed so far apart that, when the plates were slightly pressed together for a moment and left, they adhered, and yet the mooring thread[s] made an angle of 45° or 50° with each other, shewing much force tending to separate the pieces overcome by adhesion.

16009. These pieces of ice shew a fine phenomenon of adhesion. When separated and moored so that they were parallel to each other and $\frac{2}{3}$ of an inch apart, it was easy by a splinter of wood to move one round on a vertical axis and bring its corner against the face of the other; if this was done very gently so as to cause no rebound, the corner stuck, and then if the splinter were gently employed to separate the inclined piece, the second piece moved

with it. When the separating splinter was removed, the buoyancy of the two pieces caused a motion back again, and this occurred with a hinge-like motion at the sticking point; so that the inclination of the pieces to each other could be changed more or less, yet all the while the mechanical forces in action tended to separate the pieces of ice. All this could be done when the point of adhesion was elsewhere, and looked remarkably like the action of two adhesive clinging bodies.

16010. Again, if a flexible adhesive contact was established and then left undisturbed for 5 or 10 seconds–it became an inflexible adhesion; for on moving one piece *by an act of separation*, the other piece moved with it as if stiffly attached, without any change of the angle between them; but if a little more separating power was added, then the inflexible adhesion was destroyed and became a flexible adhesion, the adhering piece of ice changing its position with regard to the first but clinging to it as if attached by a hinge. Further, if the new position was retained for a few moments, it became an inflexible adhesion, and then the two would move as one mass until the force causes breaking of the adhesion from rigid to flexible, and then a new position of rest for a few moments would give a new rigid adhesion. Yet all this whilst the mechanical forces of buoyancy, push, etc. were such as tended to cause tension and separation of the two masses of ice, and when no pressure could be conceived to exist.

16011. I conceive that a flexible contact at the rounded corner of the ice was produced in this way: that the first contact caused the particle of water at the touching point to freeze, and as the flexible contact became a stiff contact in a few moments, that after a little time, particles less favourably placed than the first were able to freeze and so made the stiff contact to occur; but that if by motion the contact at the first frozen particle was broken, the other frozen and freezing particles held on, the adhesion being thus continued at the same time that it was transferred from one set of particles to another, and all this occurring under a strain causing the masses to tend to separate. Nothing, I think, can shew better that the regelation occurs when there is no pressure of ice to ice, but when there is a tendency to extension.

16012. Certain striæ were found on the inner surfaces of the ice,

caused by melting and solution, and also on the outside surfaces. These happened to be parallel to two sides of the blocks and also parallel to each other on the faces which had been in long contact. To be quite sure that they had not caused each other on the touching faces, two faces were flattened and so placed that the former striæ would, if renewed, be across each other - the pieces were then left in contact at 4h. 30′, and are to remain until to-morrow.

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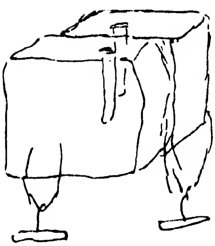
16013. The adhering pieces were looked at. Both the inner and outer faces were striated, and striated exactly in the same direction as before. Hence the striæ on opposite sides of the same piece coincided – the striæ on the two contiguous surfaces crossed each the other set and all the striæ were oblique to the perpendicular, the angle being about 45°. It is an effect of dissection by solution and shews that ice is an irregular thing - probably because of the circumstances mentioned in my Tyndall note.

P

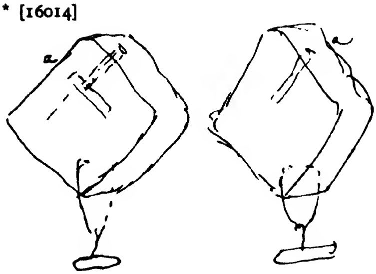


16014*. I have prepared two blocks of ice as before, with counterpoises or anchors of lead, and placed them in the ice cold water, which is kept well surrounded by ice in the pail. They were brought together, but by the insertion of slips of wood into the holes *a*, *a*, were previously placed in the position P, so that though they stuck and adhered as one piece, yet each had a strain in opposite directions round a horizontal axis. I desire to see if they will change their position and travel one on the other whilst they adhere, or whether they are strictly rigid. Left at 5.30 P.M.

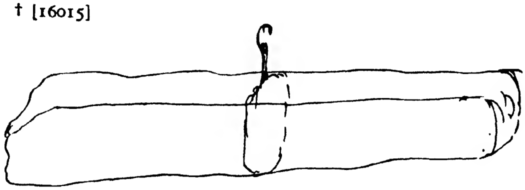
Experiments in air.

16015†. Two rough prisms of ice were prepared and touched by hot iron so as to round every angle and edge and leave a smooth surface over the whole. Wet woollen thread was tied round the middle and they were hung up by lengths of thread about 12 inches long, each from a separate retort stand, in the manner of torsion needles. When the threads had run down, but

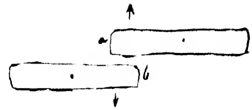
* [16014]



† [16015]



only so much as to relieve only part of the torsion, the pieces of ice were brought near each other, so that they would touch in their revolutions if they happened to come nearly in a line with each other. Then supposing that they were in the position of plan and tending to move in the direction of the arrows so as to separate; but if *a* and *b* were brought together by the slightest touch, only touching by the water, they clung to each other and held each other, first by the capillary action which supplied a moveable or hinge joint; but if left quiet a few moments, the contact became stiff, and by giving more or less vibrating motion to the whole, the unchangeable figure and therefore fixity was easy shewn and the great degree of power required to separate the pieces, considering that there was contact only at one point.



16016. All the principle facts of regelation can be shewn very well in this way in air, and though the capillary adhesion remains, the stiff contact is shewn; and then the removal of the whole into water shews that capillary action is not necessary.

16017. The effects are so strong that I have no doubt they might be obtained in common water—first the moveable joint and then the fixed adhesion.

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16018. Found the pieces of ice (16014) in the ice cold water jar in the same relative position as last night, i.e. with *no evidence* of any motion on the horizontal axis under the forces which tended to twist them.

16019. Took these two pieces and put them into a jar of water at common temperature—of course underneath the surface; found that all the effects occurring in ice cold water ensued with equal rapidity—the flexible contact at a rounded point—the rigidity ensuing in a moment or two—if the fixed joint were broken without separation and the angles of the pieces altered, then the assumption of a new angle by a fixed contact at pleasure, notwithstanding the torsion force opposing it. All these points occurred as in the ice cold water, notwithstanding the thawing that was going on at all points not in contact.

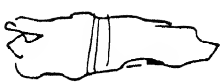
16020. Try the principle in respect of melting bodies, with Spermaceti—Sulphur—Lead—Tin—bismuth—and in respect of

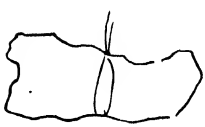
crystals and their solutions, with Glauber–Nitre–Alum–Carb. soda.

9 FEBY. 1860.

16021. *Regelation.* Experimented with Tin, having a bath of it in a ladle over a gas burner. Did very well as to the arrangement, but when trying pieces of tin, as rods, put into the bath, either when all melted or when partly melted, either with cooling and solidification or warming and melting – still could find no adhesion of the rod ends beneath the surface. This was tested however only by feeling, and in fact could hardly be expected, for Tin does not liquefy at once or solidify at once as ice. If a piece is melting, it half melts as it were, i.e. it will melt partially to the depth of half an inch or more, whilst yet the greater mass will remain in a solid condition. The portion therefore becomes like a sand of tin particles diffused through melted tin – it has no cohesion because of the liquid in it and yet much of it is solid. The same state is assumed as the liquid becomes solid, and so there is no hard point of the surface of the solidifying mass to test for regelation. The magma of crystals of tin and liquid tin would almost seem to shew that two substances were there, of different degrees of fusibility. Perhaps bismuth or sulphur may do better.

10TH FEBY. 1860.





16022. A saturated solution of Carb. Soda has been carefully prepared and preserved for some time with crystals at about 46° F. Two large crystals, which have remained some time in this solution, were suspended by loops of copper wire (fine) and thread from above, so as to act as torsion masses, and were immersed in the Saturated solution. The surface was cut by the wire and the threads were about 6 inches long. Whilst in the solution, being examined for striæ in contact with them by a ray of light passed through the whole, none could be observed, and therefore it was concluded that neither deposition nor solution of the solid salt occurred.

16023. The ends being made to come together and left in contact by a slight degree of torsion force, no adhesion of the crystals

could be observed, either at once or after 3h. 15 minutes. Being left at 5 o'clk. P.M. and re-examd. next morng. at 9 o'clk., they were found adhering; but then a very slight tendency to crystallize on the surface of the solution appeared over night, and this morning small crystals, newly formed, were over the whole surface of the crystals at intervals of $\frac{1}{16}$ or $\frac{1}{20}$ of an inch—and these had no doubt [? held] the two crystals together.

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16024. *Sulphate of copper.* Put this substance on in the same manner as the Carb. Soda (   ). It was all well arranged and there were no striæ about the crystals. There was no immediate adhesion as with ice—there was no adhesion after an hour—being left from 1.15 to 9.0, there was no adhesion—then left from 9.0 P.M. to 9 next morning, no adhesion.

16025. *Sulphate of Soda.* Good large crystals hung up—no immediate adhesion—no striæ left at 1.30 until 9 o'clk., no adhesion—being left from 9.0 P.M. until 9 o'clk. next morning, no adhesion—little crystallization at bottom of the vessel.

16026. *Yellow ferro pruss. potassa*—no striæ—no immediate adhesion—left in contact at 4 o'clk.—but at 9 they had separated; put on more torsion and left them at 9 o'clk. P.M. until 9 o'clk. next morning—no adhesion.

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16027. *Sulphate Magnesia*—no adhesion at once—or after three hours or after 24 hours.

16028. *Nitre.* The same negative result.

16029. *Alum.* The same negative result.

14 FEBY. 1860.

16030. *Borax.* The same negative result.

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16031. *Sulphate of Zinc.* In three hours there was some adhesion, but this might probably be an appearance due to the direction of the torsion force. Left all night—no results next day.

16032. *Acetate of lead*
16033. *Muriate ammonia*
16034. *Rochelle Salt* — Same negative results.
16035. *Nitrate of lead*
16036. *Nitrate of soda*

16037. *Lead.* I tried lead as tin was tried (16021), but with the same negative result.

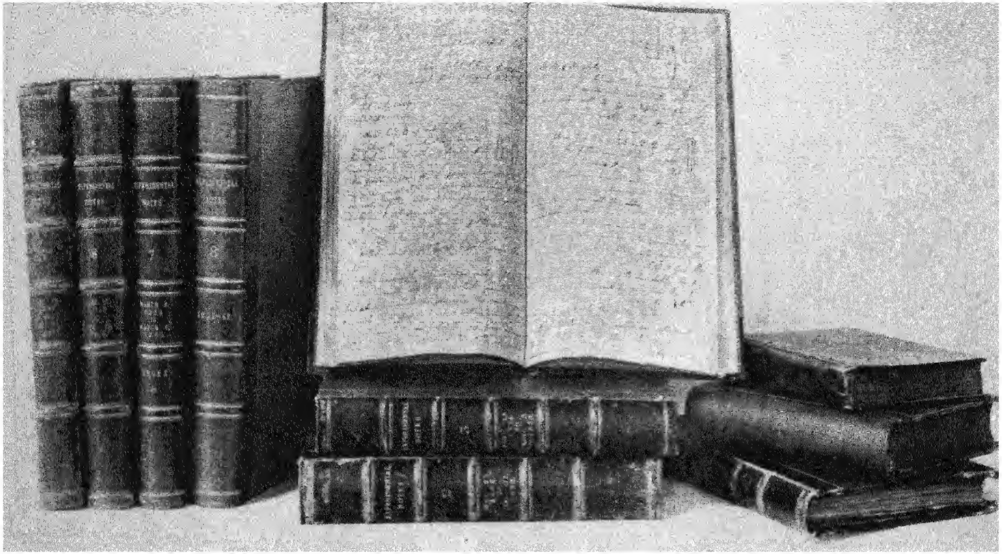
6 MARCH 1860.

16038. *Glacial Acetic acid.* A bottle containing about 4 or 5 oz. of this acid in the fluid state was at such a temperature that, being disturbed by a glass rod, the acid began slowly to form crystals which went on increasing in size and number for several hours. But during this time there were no signs of regelation. The crystals when pressed together did not adhere as ice crystals do—but slid easily one over another and rather shewed an indisposition to unite and a sort of greasiness in their own fluid, for it tended to withdraw from their own surfaces. At the end of 12 hours, about one half of the acid remained fluid amongst the crystals of the other half. No doubt a little lower temperature and time would render all solid.

16039. *Nitre.* Nitre was fuzed—then rods of nitre were put into it whilst the temperature was just above the melting point. When they had acquired the temperature of fusion and were brought together, they sometimes seemed to give slight evidence of regelation; but the point is doubtful, and may have been due to the internal temperature of the rods not being quite up to the melting point. Nitre is like water in this respect, that its bulk is larger in the solid than in the fluid state.

16040. *Bismuth.* Was melted in a ladle over a gas burner and tried. No signs of regelation. When the half solid, half fluid mass was broken up into small crystals in liquid by agitation—there was no indication that the crystals could cohere like snow or ice in water. When pressed together, they rubbed loosely just like grains of sand.

16041. *Ice.* Cooled carelessly and only a few degrees below 32°—had lost all signs of regelation or of any tendency to adhere. Pieces offered together in the air shewed no signs of adhesion until the temperature had risen so far as to allow moisture to appear on the surface, and then regelation took place.



THE BOUND VOLUMES OF FARADAY'S DIARY

FOLIO VOLUME VIII
OF MANUSCRIPT

TUESDAY. 4 OCTR. 1853. *Telegraph Wires*[1].

Experiments made at the Gutta Percha works on the charge taken up by wire covered with Gutta Percha when immersed in water. Shewn me by Mr *Latimer Clark* and Mr *Statham*, of the works.

All the wire is copper and of the same diameter, being No. 16 and about $\frac{1}{16}$ of an inch in diameter. The covering of Gutta Percha as of two thicknesses, making the one wire No. 3 or $\frac{8}{32}$ of an inch in diameter, and the other No. 4 and $\frac{7}{32}$ of inch in diameter. The junctions of the covered wire are carefully made by overlaying the ends, covering with a fine binding wire, soldering, and then carefully covering up with Gutta Percha. Other junctions at the ends of coils are made by twisting ends together. Coils of covered wire (each containing $\frac{1}{2}$ mile) were hung from the sides of two barges in the canal, into the water, so as to be submerged except at the wire ends of each coil, which were in the air, twisted together, and covered with Gutta percha cones to keep off damp. In this way 100 miles of covered wire was immersed in the water. 120 miles of like covered wire were arranged in a loft above in one series in the *air*—for a contrast or comparison with the immersed wire.

The Galvanometer used was either a small upright arrangement called a *detector*, or a larger instrument, being a regular galvanometer. The detector was sufficiently sensible when in the 100 mile circuit to shew deflection when the only source of Electricity was a *slip of zinc*, the *copper wire end* and the *tongue*. That no short cut for the current was open was shewn by a test experiment to be described directly.

In the experiments, batteries were used consisting of amalgamated Zinc and of copper plates in sand damped with dilute Sulphuric acid. The troughs were of Gutta Percha and therefore the contents of each cell insulated. All the connecting wires were covered with Gutta Percha and therefore insulated. The plates were simple and 4 inches square. The usual number of pairs employed was 360. Of course, the force of this battery sent through the 100 miles in water and air and the *detector* or the instrument

[1] See note in square brackets on p. 411.

gave powerful deflection. But if the twisted connexions in any part of the 100 miles were opened, the current stopped–shewing that the whole 100 miles were in the circuit–and also that the whole of the circuit was well insulated.

Mr Statham has a very interesting Electric fuze. On a certain occasion[1] some Gutta Percha was sulphuretted or mixed with sulphur and then employed to cover copper wire. After being kept several months, it was found on stripping off the Gutta percha that it brought away with it a layer or coat of Sulphuret of copper, formed by the metal and the sulphur in the Gutta Percha. If a half cylinder of such Gutta percha have wires laid in it* so that the ends shall be above $\frac{1}{3}$ of an inch apart, even a feeble battery current[2] having intensity will pass by the sulphuret and will cause ignition of its particles here and there, and such a fuze will easily fire gunpowder. This ignition may be repeated 5 or 6 times with the same fuze before it loses its power, and is obtained by a battery force far too feeble to ignite a platina wire. Another form is to pull the wire out of the tube–to cut open the middle part of the tube†–to introduce two fresh clean wires as in the figure firing the sulphuret at the opening or intermission of wire.

When the 360 pr. of plates current was sent through the 100 miles of water wire and one of these fuzes, the sulphuret was instantly ignited. In this way a cartridge of Gunpowder was exploded under water by one wire only, the other wire from the cartridge going to a 4 inch copper plate making contact with the water or earth. Contact with the earth is sufficient in these cases of insulated wires and batteries, i.e. contact at one end of the battery and at the other end of the wire.

The fuze seems to conduct better gradually, for when a small power is used, it gradually rises up to ignition, requiring a second or two for the purpose.

This battery of 360 pr. plates affects the body well, but I doubt whether there is much quantity. Its intensity is considerable.

[1] It was at one time *customary* to use wire covered with sulphuretted gutta percha (Latimer Clark).

[2] "Feeble" battery current. I believe that at least 144 cells of an intensity current are required–quantity is not necessary (Latimer Clark).

*

†

Absolute charge of water wire.

The two ends of the 100 miles of water wire were brought into the house, one end of the battery was connected with the earth (an 8 inch square copper plate in the canal water being the connexion), and the other end of the battery connected for a moment with either end of the water wire. Then one end of a fuze being connected with the earth, either end of the water wire was connected with the other fuze end, and the sulphuret ignited; shewing that the immersed insulated wire had taken a charge and when insulated could keep it—and could then give it out to the fuze and ignite it. An intensity battery is requisite for this purpose and it must be insulated—quantity battery is not required. The action on the fuze is progressive, i.e. the discharge is not instantaneous and the fuze takes time to ignite. Either end of the wire will fire the fuze.

In this case the insulated wire has taken a charge upon the common principles of induction. The whole wire has become charged as the end of the battery connected with it—the outside water takes by induction the opposite state and the gutta percha is the insulating dielectric thrgh. which induction takes place. The enormous extent of wire is the cause why so much electricity is stored up in the static state, to produce a current as it returns. The whole inducing surface of the 100 miles of $\frac{1}{16}$ wire is 8272 square feet, acting across gutta percha only $\frac{1}{11}$ or $\frac{1}{10}$ of an inch in thickness.

The fact is a beautiful case of the identity of static and dynamic electricity and is only a variation of Davy's experiment of charging a Leyden battery by a Voltaic battery.

Sensation. When the water wire is charged and then discharged through the *uninsulated* body, a powerful electric effect is felt, a regular voltaic battery effect, but only for a moment. The discharge is not instantaneous. By tapping the end of the charged wire against the uninsulated finger I could break up the one discharge into many smaller *successive* discharges. I could obtain even 40 little distinct shocks this way. Here the necessity of time for the discharge is beautifully manifest—due partly to the resistance to conduction in the body, the outer coating water and

the wire itself, the latter being considerable, and perhaps in part to a partial penetration of the Gutta percha by the electric charge.

This same charge of the *Water wire* affects the Galvanometer or detector and that very strongly for an instant. As with the shocks, so a series of effects can be obtained, shewing the point of *time*.

Here also was shewn the retention of the charge by the wire, for when it had been charged as described, by contact with one end of the battery for 2 or 3 seconds, then separated and then left for an interval before brought in contact with the Galvanometer, still at last it deflected it upon contact, though less powerfully as the interval of *time* was longer. After counting 1, 2, 3, 5 or 10 the action was considerable. It deflected the instrument after an interval of 3 minutes and even after 5 minutes.

Now the 140[1] miles of like covered wire not immersed in the water but surrounded by air was equally perfect in its insulation and also as a continuous conductor—but it shewed no phenomena of charge. When brought into contact with the end of the insulated battery and then separated, it was utterly unable to affect in the slightest degree either the fuze or the body or the Galvanometer.

This wire having air only next the Gutta percha could not take up a charge; it could induce only to the distant surrounding walls of the loft, and to them only by the general surface of the whole heap of coils, equal perhaps to a cube of 8 or 10 feet in the side and not to the sum of the superfices of the wire; the supposed cube, too, being many feet from the walls of the building, whereas in the water the conducting coatings were only $\frac{1}{10}$ of an inch apart.

On a particular occasion, 20 miles of wire covered with Gutta Percha was afterwards covered with lead—and this acted equally well with 20 miles of the water wire—the outer lead coating being the second surface in the induction. The wire was just as well insulated as in any of the former cases.

Now a Galvanometer, insulated, had the water wire put in contact at one end of its coils, and then one pole of the battery (the other pole to the earth) placed in contact with the other Galvanometer end. Immediately there was a great rush through the Galvanometer, but contact being continued, this rush ceased

[1] Should have been corrected to 120.

when the water wire was charged and a permanent deflection of 7° or 8° only remained. The first rush was due to the run into the water wire; the 7° or 8° permanent was due to the leak of electricity in the course of the 100 miles of wire, i.e. conduction away either through the Gutta percha or at the twisted contacts or elsewhere above. This leak is of course under the influence of the full tension of the battery of 360 plates.

Then the battery was disconnected with the Galvanometer and replaced by an earth wire contact: immediately there was a powerful deflection like the former, due to the return of the electricity laid up for the time in the wire, and then all effects ceased for the time, the wire being discharged. Current into and out of the wire and its assumption and loss of the Static form beautifully shewn here.

When the 120 miles of air wire connected in place of the water wire–no effect of this kind occurred.

There were two like telegraph instruments or Galvanometers. One of these was connected with the 100 miles of wire in the water, and the other with 100 miles of the air wire. The ends of these two lengths were connected together and with the earth, and the other two ends of the Galvanometer were connected together and to one pole of the battery, of which the other was to the earth. The effects were equal, and that in every way in which the two wires were changed in respect of the Galvanometer. In fact, as to the carrying of a constant current, the two lengths in air and water *were alike*, notwithstanding a difference of induction which must have existed in some degree.

Mr Clarke and myself then went to the Telegraph office at Lothbury, for the purpose of seeing some effects due to this action of induction, in wires laid underground between London and Liverpool. The wires are coated with Gutta Percha and then as many as eight go all the way, each being wrapped about with tow and then enclosed in tubes, sometimes earthenware, sometimes iron. These tubes containing the eight wires are about 3 inches in diameter, and Mr Clark has little doubt that all the eight wires are nearly as the 200 miles of wire in the canal[1]. As the

[1] The meaning of this is obscure–the insulation is not so perfect as that of new wire–100 miles give 25° instead of 8° or 10° (Latimer Clark).

length of each wire is about 200 miles, so by combining two or more together great lengths can be obtained.

Now effects of this kind occur. Connecting London and Liverpool by two earth wires, so as to have metallic circuit all the way—or by two air wires, so as to have like metallic circuit—when the signal is made by putting on the current at London, a like signal occurs at Liverpool in the instrument there—it is quick and ready if the air lines be used and the weather fine, so as to give good insulation—but it is slower and duller by the earth lines, and as they tell me, when the insulation is as good or better than that of the earth[1] lines. The power is transferred sluggishly, as if the induction disposed of it for a time—the wave of power passes more slowly along the subterranean line than the air line—its elasticity or propelling force is now disposed of in an exertion of lateral inductive force.

This fact they tell me is of serious consequence and would be of great importance in long submarine telegraph wires.

It was shewn to me by putting four of the wires in connexion with three instruments, as in the diagram*, thus having 800 miles of wire, with two instruments near to and on opposite sides of the battery and a third instrument in the middle, or by the wire 400 miles off. When a signal was given at the battery instruments, they moved instantly and strongly, but the instrument at half distance moved sensibly after the others and slowly and not so much.

It was not difficult to make a signal so quickly that whilst very distinct at London it was not sensible at all at Liverpool. To understand this, it must be remembered that in an instrument the handles hang perpendicularly or are jerked a little way right or left. Whilst they hang perpendicularly the instrument helix is not in the voltaic circuit and the end meant to receive the battery communication is connected with the earth. A motion of the hand first cuts off the earth and then completes battery communication; and when the hand comes back, battery communication with the helix is cut off and earth communication restored. So that any electricity stored up in the earth wire by induction can come back again by the instrument to the earth, and does produce a reverse

[1] ? air.

*

current or revulsive action, which knocks the needle back with a power far surpassing its tendency to fall back to the perpendicular when that earth communication is cut off or prevented at the time the battery connexion is cut off.

So when a quick to and fro motion was made at London as mentioned above, the *to* motion caused separation from the earth and battery contact with the wire at London, and the *fro* motion caused separation from the battery and earth contact at the same place; and this so quickly that the charge given to the wire by the ingoing electricity at the *to* motion had not time to pass to Liverpool altogether, but came back in part at the *fro* motion to the earth in London, and gave the extra impulse backwards on the needle spoken of. This knock back does not take place with the *air* wires it shews the *charge* of the earth wire by the supplemental current due to the charge itself, and it shews the *time* taken in the transmission by the earth laid wires—and the cause why the Liverpool signal is both weaker and later than the London signal.

By very quick motions it was possible to shew the successive waves of Electric force along the wires to the instrument 400 miles off. Two such quick motions, by management, could be made to shew the second signal in London occurring at the same time with the weaker first signal at the 400 miles—or a reverse signal at London to that at the 400 miles—halving as it were the way—or causing the laid up induced static electricity to flow out in opposite directions at both ends of the 400 miles.

Such effects must interfere very much with the determination of the *velocity of electricity* in wires, when the wires are submarine or subterranean, and have probably produced some of the discrepancies in measurements of this velocity by different persons. It must also interfere very much in the determination of longitudes between places far apart. Two places on the same meridian might appear east or west of each other according as the observations are made from one end or the other—and if part of the wire was in air and part in water, the differences might appear to be unlike at the opposite ends.

The *reaction* back upon the instrument near the battery when the earth contact is removed I have spoken of. It is due to the

return of the induced electricity. A little of it is not objectionable, for it gives quickness to the motion of the needle. But much of it knocks the needle back with so much force that it is too long before it settles, and quickness of communication is interfered with, for the talker or reader requires more time between the signals. Some practical means of preventing this action is required.

Then we saw some effects of contact with the earth at different places—but of these results I am doubtful, for I am not sure of the condition of the battery, whether insulated or not, and had to take very much for granted as to the conditions of the wires, etc.

When the circuit was 400 miles of subterranean wire returning to London but the end left insulated*, then contact of the battery with the Instrument cause[d] a slight constant deflection of the needle, shewing a slight discharge in the course of the 400 miles and its amount.

When the end of the 400 miles in London was connected with a chemical printing apparatus (Bain's), where marks of prussian blue were produced on a damp properly prepared paper†, these marks, which were produced every time battery contact was completed in London, should appear as straight lines, longer or shorter according to the time of contact but well terminated at both ends, thus — — - - - - —— - - - - — — — ; and so they do appear with a *short* wire or with an *air* wire. But with the earth wire the marks all have tails and also gradually increasing commencements, thus —— —— —. As the paper travels with a uniform velocity, these shew the time occupied before the full current is on, by the raising of the induction, and they also shew the time the current continues to fall off after battery communication ceases. By making a series of quick touches at the battery, the marks would appear thus— — — — — —. By making one quick touch, it could be seen that the mark was made distinctly after the touch was over and not simultaneously with it, the wave of power having run through the 400 miles of wire and existed for a time in it by *itself*. So the successive lines above shew the successive waves running on in the wire.

By making the battery touches very rapidly, it was easy to obtain a *continuous line*, shewing in this way a series of rapid *intermitting* touches of high intensity converted into a continuous

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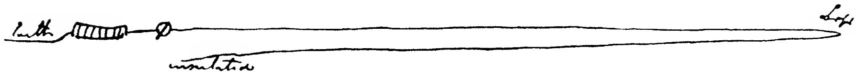

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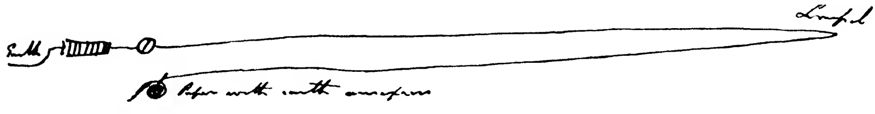

and nearly constant current by the *induction time* of 400 miles of wire.

How extremes meet! It is just the case of the equalization of force by a fly wheel of many tons in weight moving with an Engine of 500 horse power. Such actions, however, interfere with the quick use of the instrument.

SAT. 15 OCTR. 1853.

At Lothbury Telegraph Office from 5 o'clk. to 9 o'clk. P.M. leisure time. Present: Mr. L. Clark—also Airy, self, E. Clark, Mr Varley and workmen.

Had all the eight underground wires to Manchester at our disposal: the distance is 192 miles, and the whole sum of wire 1590 miles of wire.

The Earth contact at Lothbury station is a hollow copper sphere two feet in diameter, well buried. It is filled with a solution of sulphate of copper, but that is valueless in effect—was meant to be reduced by the currents but can have no such power.

The battery was in the Vaults at Lothbury, and not insulated otherwise than by being in dry places on dry shelves of wood. There were 508[1] pairs of Zinc and copper plates—averaging 4 inches square[2]—excited on the zinc side, amalgamated, by sat. sol. of sulphate of Zinc and on the copper side by saturated sol. sul. copper—the fluids being separated by porous diaphragms; copper is gradually deposited, zinc gradually dissolved, but action is very slow; crystals of Sul. copper are gradually added. The impervious divisions are of slate, and these with the inside of the trough are covered with marine glue—hence an aid in the insulation.

The battery has considerable intensity but not much quantity. The shock to one or to five persons is very considerable and continuous—but the action on acidulated water very small. They like it better than the sand batteries.

The instruments employed have no other insulation than that

[1] Altered by Faraday from 288.

[2] After the first experiment 508 cells were employed—the current you received through the hands was from 508 cells—some of the plates (about $\frac{1}{5}$th of them) were only 4 in. by 2 in. instead of square (Latimer Clark).

afforded by the dry mahogany wood, of which the supporting parts are made.

The first arrangement was as follows*: *w*, *w*, *w*, are the eight insulated subterrene wires connected metallically in pairs at the Manchester ends, and also as shewn at the London ends; but with three like instruments of indication interposed as shewn at A, B, C. The whole length of wire therefore is near upon 1600 miles. The battery whose current is to be sent into it was placed at the A end, and one of its poles properly connected with the earth; and the extreme end of the wire at C was also connected with the earth. Whether connected with the earth at London or at Manchester or (if possible) at the extreme distance of 1600 miles, is supposed to make no difference, for no difference due to such a variation (in kind) has yet been discovered.

Of the Galvanometers or detecting instruments therefore, that marked A is close to the battery—that marked B is 800 miles off and that marked C 1600 miles away, as regards the conducting wire.

When the proper battery contact was made at A, the A needle moved instantly with a quick strong action: the B needle moved about a second after A, with much less force, and its motion was slower. The C needle was about a second after B before the motion came on—then it was very feeble—and it took a full second to rise up to the maximum deflection. Each of the galvanometers B, C, was later, feebler, slower and finally weaker in action than its predecessors—the electric current being sent into the wire the whole time.

On breaking contact, the needle A was knocked back powerfully and quickly: the needle B was later in returning and fell back into its natural position easily: the needle C was still later in returning and returned slowly.

In all these effects, there is the evidence first of the storing up of Electricity in the wire by the conversion of current into static electricity, consequent upon an effect of induction across or through the gutta percha which covers the whole wire; and then, after battery contact is broken, of the reconversion of the static into current electricity. The continued deflection of the needle C, after the battery contact is broken, and the simultaneous revulsion or

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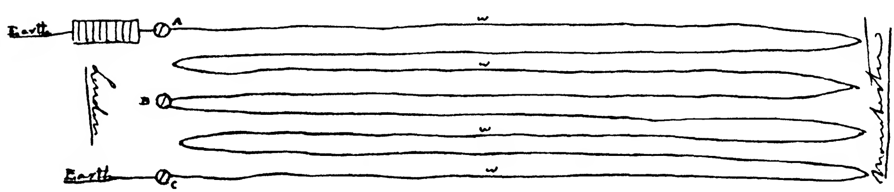

knock back of the needle A, are both due to the *same charge* of electricity, which is then flowing out of the two ends of the wire.

The condition of *time* is also very manifest in both sets of phenomena.

The charged condition of the wire was shewn also in this way. The wire end was separated from Galvanometer A. Next, the wire end was placed in contact with the battery pole, to charge it–then separated from the battery and afterwards applied to the instrument; and the needle was deflected by the charge which the wire still retained and carried to it.

It must be born in mind that this wire is by no means completely insulated. This was shewn by separating the end C and leaving it insulated in the air: then when battery contact was made with A, it was permanently much deflected, and B also was permanently and considerably deflected. These effects could only happen through the dispersion of electricity in the course of the wire–and hence the reason why, when all three instruments are connected and the last or C with the earth, the current is far greater through A than B–and thrgh. B than C. I do not think more than a tenth or twentieth of the electricity which has passed through A goes on to C.

By making a very rapid short touch with the battery at A, a very strong *direct* and *reverse* motion could be obtained there without any sensible effect at C or even at B. The electricity went into the wire and came back again thrgh. A into the earth, there not having been time for a sensible portion of it to go on to C or B.

By a touch, quick, but a little longer than the last, B could be deflected; and because of the time required, this could be effected *after* the contact at A was broken; the wave of power rolling on from A to B and not reaching B until after it had ceased at A–the tide had sometimes even turned there, as was shewn by the revulsion or back action.

When the electric current was passed through A and continued, and the dial face turned until the needle took up its deflected position free of the restraining pins, it was seen that, at the breaking of electric contact, the needle was for a moment deflected

still more strongly than before, after which the revulsion took place. This extra and intermediate deflexion is due to the increased force of the current at the moment of breaking contact, which act, as I shewed long ago, is able to induce a current in the same direction in a *neighbouring* wire or to increase the force of the current itself in its *own* wire.

When instead of the arrangement (p. 10[1]), the earth was separated from the battery at A and also from the extreme end of the wire at C, and the battery and C wire joined together and insulated from the earth*: then upon contact at A, the instruments A and C move together and at once and are also upon breaking contact revulsed simultaneously. B, which is 800 miles off by the wire, acts after a time—more slowly and has no revulsion.

Now the end *c* of the battery, instead of drawing upon the earth, draws or acts at once upon the instrument and wire at C, and with an amount of force equal in all respects to that exerted at A.

An air wire communication to Liverpool and back was employed; it was only 400 miles long. None of the above effects were observed in a sensible degree. With 400 miles of underground wire, they are all very evident.

Proceeded to employ Bain's printing apparatus. In this arrangement, the end C of the telegraph wire is a fine iron wire, which rests on the edge of a cylinder about $2\frac{1}{2}$ inches diameter and half an inch long, which revolves on a horizontal axis. This cylinder is connected with the earth, and being a conductor, completes the electric circuit. It is connected with clock work which causes it to revolve with a regular velocity; and a second revolving cylinder bears against it, so that the two in revolving can clip the end of a long band of paper $\frac{1}{2}$ inch wide and draw it gradually onwards. Such a slip, many yards in length, is wound on to a bobbin or pin and held loosely on its axis so as to unwind with little force; and being first imbued or rendered slightly damp with a mixed solution of yellow ferro prus: potassa and nitrate of ammonia, and can thus be drawn regularly between the iron point and revolving cylinder. The electric current is so directed

[1] See p. 402.

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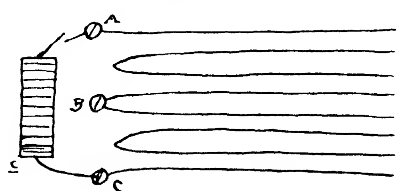

that the iron point shall be positive, and therefore whenever it is passing it a deep blue line is formed under the iron wire upon the moving paper. As the paper moves with uniform velocity, the marks so produced are either mere dots or lines of different length, just as the communicant has continued the electric contact for a longer or shorter time.

In the experimental arrangement, three iron terminals or pens were employed, placed in a row*, each making its own mark upon the paper when the electric current passed through it. *a* was a short wire, a few yards only in length, connected with a battery of 24 pairs of plates only: it was connected with a contact instrument, and being perfect and instantaneous in its action, it shewed by a blue line (to be called *a* on the experimental papers or strips)[1] when the electric current was passing and when not.

The wire or pen *c* is the end of the 1600 miles of earth wire, and it of course shews when the current of the 508 pairs of plates at the A end of the line has made itself manifest at the C end: this long wire is connected with the contact machine so that the battery contact with it is simultaneous with that of the small battery with its short wire. The two currents therefore are started at the same instant, but that of *a* has to go only through a few yards of air wire whilst that of *b* has to pass through 1600 miles of earth wire. The breaking of contact at the batteries is also simultaneous for the two.

The wire or pen *b* is associated with the A or battery end of the 1600 miles of wire and also with the contact machine—but in such a way that it is insulated or out of contact with the battery and wire when they are *in contact* with each other; but upon breaking this contact (See Clarke's diagram†[2]), it is at the same

[1] The records from the printing apparatus referred to here and subsequently are not included or illustrated in the MS.

[2] Drawn by Mr Latimer Clark and bound into the MS.

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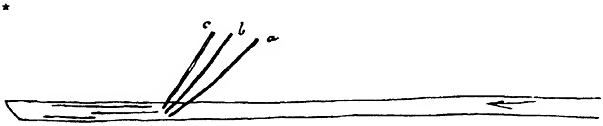

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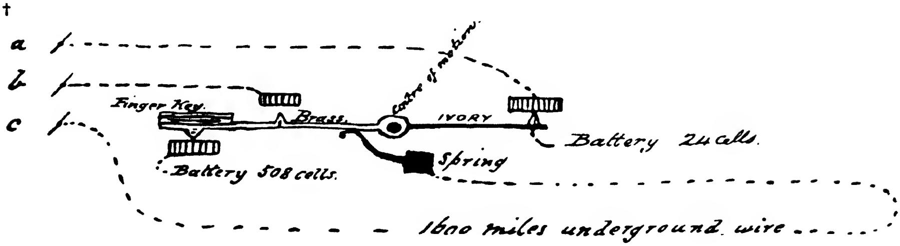


instant put into contact with the A end of the long wire. Hence on putting down the contact key, the battery current can flow into A end of the earth wire, through it and out at *c*, making its mark, and on lifting up the finger, any charge contained in the wire can flow out either at *c* or *b*, being manifested by corresponding marks.

With the small battery, short wire and pen *a*, the effect was perfect. The blue lines were of equal thickness and intensity throughout, beginning and ending sharply; perfectly separated dots could be marked close to each other, the most rapid touch of the finger on the contact key being accurately told. (See Experimental paper M and also N and O.) Rattling contacts could not be run into a continuous line, so strict was the indication. Hence the mark made by this pen *a* will be a true record of the time when any current from the great battery is running into the 1600 miles of wire at its battery end A.

Then with the great battery and wire of 1600 miles. There was a sensible time between the battery contact at A and the appearance of any effect at the end of the wire or *c*–a full second intervened–then the line made by *a* (with continuance of battery contact) rose gradually to its maximum condition of intensity, remaining always a feeble line because of the little electricity which travelled to the extreme end of the wire. Upon breaking contact the mark of *c* did not cease suddenly (as that of *a* did) but was continued for a couple of seconds or more, being due now to the electricity flowing out of the charged [wire]. The wire charged by induction continued to discharge in current form at the *c* end (see Exp. papers marked P, Q, R and S).

Then for the revulsion or return of the electricity at the A or battery end of the wire when contact is broken with the battery and made with the earth. For this result the C end and therefore pen *c* was connected with the ground, or else C insulated (the results being alike in kind and nearly in degree because of the little electricity which goes to the end of the 1600 miles of wire), and then battery contact made for a time and then broken at the instant earth contact by the printing machine was made at A. The consequence was that the moment this was done, *b* printed a very strong line gradually diminishing to nothing (See Exp.

paper T). This line is due to the charge returning out of the wire after the battery is off. It may be seen at the beginning that a very short battery contact gave a certain portion of return electricity, a longer contact more, but when a certain time of charge has been employed, then the wire has acquired its full charge and a longer contact of battery does not increase the effect. The time of the charge is measured by the length of the *a* lines from *x* to *y*–the amount of revulsion by the *b* lines.

A number of 10″ results were obtained when the wire *c* was in use, i.e. when the pen at the end of the 1600 miles was doing duty. They are on the Experimental papers P, Q, R and S. The contact key was down for 10″, then up for 10″, then down for 10″, up again for 10″ [1]; so that the distance from 1 or *x* to *y* is the 10″ of battery current and that from 1 or *x* to 2 the whole 20″. The slowness of the coming on and going off of the effect at *c* or the end of the wire is well seen, and the weakness of the maximum effect and the continuance after the battery current is stopped. The power of the return current at *b* is also shewn; it is so intense at first that a spark even appears and the paper is burnt for a little time. The duration of this current here appears long, up to 4″, 6″ or even 8″, but that depends in part upon the imperfect conduction of the paper. If the pen *b* touched metal connected with the earth, the escape of the stored up electricity would be much quicker. The way too in which, after battery contact is broken, the electricity runs out of the charged wire at both ends, or *b* and *c* simultaneously, is very evident–and the reason why the *c* mark is so much weaker than the *b* mark for that time is that the near or A end of the 1600 miles of wire is that which is charged most highly, for a stronger current has gone into it than into the further parts.

In consequence of the charge taken up and held for a time by the wire, it could be used analogically as a storehouse or flywheel, and the electricity thrown into it in separate portions could be converted into a continuous stream. Thus if contact was made

[1] The key was alternately down for *ten* seconds, and up for *ten* seconds, and so on–not *five* seconds. The paper travelled about 2 feet per minute (Latimer Clark).

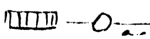
The times in this paragraph are as corrected by Faraday.

and broken for equal short intervals, then alternate marks appeared from the pens *a* and *b*—at *a* for the times of contact—at *b* for the times of non contact or the times of the returning current. No return current could appear at *b* whilst *a* was in contact with the battery, for then the *b* pen was separated from the wire (See Exp. paper O). But if the contacts were extremely short and the intervals somewhat long, then the line produced by *b* or the return electricity was almost continuous (See exp. paper N), and if *b* had been put into permanent contact with the further end of the earth wire, then the line would have [been] quite continuous.

Now 500 miles of air wire between London and Liverpool were experimented with, of which about 20 are underground or in close approximation to tunnel walls, etc. Here there was the smallest possible trace of effect at the *b* wire of stored up electricity, that being due to the underground part; and so the induction with air wire was proved to be insensible (See Exp. paper U). The contingent rapidity of transfer was shewn by the Galvanometer instruments. It may be seen however that *c* is after *a*, shewing the time occupied here. I believe the current was continued for a second, and I think Airy judges the distance to correspond to about $\frac{1}{15}$ of a second or thereaway.

Thick wire—help in curing the slowness of transmission.

A *momentary contact with the earth* between the wire and the instrument, near the instrument, ought to cure the reaction back. Done by causing handle to make contact first with instrument—then with earth at *a*—then to break with instrument—then with earth at *a*.



27 OCTR. 1853.

As to the contacts between instrument and battery*. *m* is a cylinder to which the ordinary handle is attached; it consists of three pieces, the two ends metal, the middle wood—the C end is connected by a spring with the copper end of the battery and the Z end with the zinc end—the C end has a metal pin C attached to it and the Z end an upright metal pin Z affixed to it also—*n*, *n* are two blocks of metal; *b* and *c*, two metal springs rising up from them and pressing on the fixed piece of metal *a*. The blocks *n*, *n*

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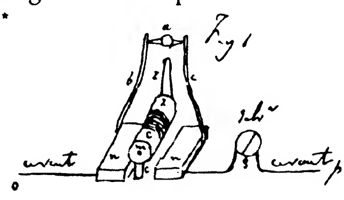


are connected with the telegraph circuit—including the galvanometer or instrument, as seen. In the quiescent state therefore, Fig. 1, the circuit is complete by *o*, *n*, *b*, *a*, *c*, *n*, *g*, *p*, etc., including the galvanometer but no battery: for the battery ends hang between at C, Z but out of contact. But when the handle below is carried to the left, as in Fig. 2, the upper pin Z bears against *c* spring first and then the lower pin C against *n*, *b*, which action opens *c* from *a*, thus interrupting the circuit there, and then completes the circuit again through the battery ZC, thus giving the electric indication. On bringing the handle back, the battery is taken out of connexion and replaced by the conducting piece *c*. On making the handle move to the right hand—the current is again sent through the circuit in the contrary direction.

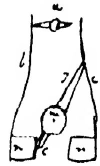


Fig 2

So there are six steps in the completion of one signal. This in fig. 2. The Z end of battery is connected with spring *c*—then *c* is disconnected from *a*—and then the C end of the battery is connected with *b*: in that state an electric current runs through the circuit and galvanometer. On returning the hand[le], C first leaves *n*, *b* and so one end of the battery is taken off and no more current runs then *c* closes on *a* and the circuit is complete for signals from the other end or station, and finally Z leaves *c* and then both ends of the battery are insulated.

DECR. 23, 1853.

Some experiments at home with Mr Statham's fuze.

A Grove's battery of 10 pr. plates would not ignite it—not intensity enough. An inductive apparatus urged by 1 or 10 pr. of Grove's plate would not ignite it. Sparks passed at little interruptions with either the 1 or 10 pr. but there was not quantity enough to ignite the sulphuret—plenty of intensity.

A Saxton's Magneto Electric machine would not ignite it.

A Leyden battery of 15 large jars were connected with the ends of the fuze and of the Saxton coil, but no change.

The same Leyden battery connected with the prime wire of the inductive apparatus and the 1 or 10 pr. of Grove's; but no ignition.

The same Leyden battery charged well up by the E. Machine

and then discharged through a wet string and the fuze, caused no ignition of the fuze.

A Leyden Jar–the same result. If the string were away, then even a small charge of the jar appeared as a bright spark at the fuze; but only as an electric spark, not as ignition of the fuze.

Charged a battery of one inch plates, zinc and copper, with dilute Sulc. Acid–insulated 300 pairs; the shock on wet fingers very sensible. Still it did not ignite the fuze–not quantity enough and perhaps not intensity enough.

TUESDAY, JANY. 17, 1854.

With Mr L. Clarke and Mr [illegible][1] at the Wharf.

Two miles of wire (covered G.P.) in different coils in three tubs of water–the tubs themselves being insulated on sheet Gutta Percha.

A battery of 12 × 24 or 288 pr. of plates, sand and S.A. diluted–also insulated but one end connected with earth–the other end for use. This battery had very feeble chemical action on water–but good intensity and gave a good shock.

Wire ends all connected together and with a detector instrument the tubs and water as yet insulated then battery end up to the deflector, but no deflection because tubs were insulated and no induction or entrance could occur.

Water and tubs connected with earth–then battery end to the deflector: was affected for a moment and then stood at zero–shewing entrance of Electricity to charge the wire and water–and also that when charged there was no continuing leak.

Then quickly separating battery from detector and touching the latter with earth wire, the reverse deflection occurred, shewing the return of the electricity or discharge of the battery.

The wire was charged–then battery separated and the hand quickly applied to the wire to discharge it–a sensible shock was felt. It was not enough to ignite Statham's fuze.

The water was insulated–and then uninsulated through the hand and body. Then each time that the battery wire was applied to charge the wire–a shock was felt in the hand–and each time

[1] Possibly Wallis.

the wire was discharged the shock of the return was again felt by the hand.

The two miles of wire, instead of being as one wire, was arranged as two Leyden apparatus in succession or cascade. When the first was charged by the battery—the hand uninsulating the water of the last felt the shock—and also on discharge.

When the wire was in a series of three, namely a mile and two half miles—still the same result was sensible.

The whole length of wire was not enough to shew sensible influence on time.

By using the horizontal Galvanometer and adding the impulses, the swing soon rose up to 90°. So a very evident result.

[A page of corrections and comments by Mr Latimer Clark on the foregoing account of the experiments on Telegraph Wires is bound in to the MS. here. The corrections have generally been made by Faraday in the text. The comments have been added as footnotes with Mr Clark's name attached.]

23 JANY. 1858.

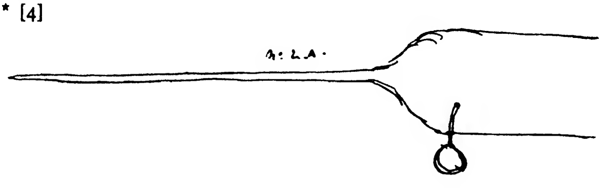
1. *At Mr Gassiot's.* Saw his fine experiments on the luminous striæ of the Electric discharge, especially with his tubes, and above all with the four horizontal tubes one over the other. Of the phenomena here presented, the following seem to me important: The well developed alternations of light and darkness at intervals of $\frac{3}{4}$ of an inch or more. The displacement of them by the fingers travelling along the outside. The charged condition of the outside, giving luminous brushes to the finger or a conductor applied there. The continued recurrence of charge and discharge there, shewn by the continual recurrence of the brushes. The probable intermittence of the charged state corresponding with each recurrence of the luminous state. The probable difference in *charged condition* of the part opposite an obscure space and a luminous space. The capability of applying conducting rings or coatings round the tube at the obscure and the dark spaces and getting indications of the kind of charge by a spark to a little Leyden phial. The capability of moving the luminous spaces by moving these ring coatings. The relation of the coating of a dark place and a luminous place to each other.

2. The influence of the magnet—first in *deflecting* the whole discharge whilst the alternations remain unchanged—next in determining the luminous places; for as the magnet horseshoe moved along the tube, the luminous column with its alternations moved with it—so that large drops or globules of light could be drawn out of the platina wire or sent back into it. The fixed relation of the places of light and darkness to the lines of magnetic force. A piece of metal, copper, did not carry these luminous spaces along in the same way--nor did the bend of the horseshoe magnet where the magnetism was weak.

3. A *single* discharge of the Ruhmkorf can produce the alternation. I am told a *single* discharge of a Leyden jar can do it.

4*. In the horizontal tubes—there is a prolonged end, small and fine. Whenever the luminous alternations appear, this part is filled with like light and I believe truly represents a luminous space—luminous from the same cause. As this is not due to a

* [4]



through current, so I suspect the other places are not due to a through current necessarily, but to a *condition* due to *charge and discharge*; whether the discharge be forward or backward, forwd. in the course of the current by intermittances, but backwd. in the thin part by alternations. The proposed coating would probably shew this.

5. The luminous and obscure parts are of course visible *only* whilst the discharge is going on, and therefore they intermit, occurring at each passage whether these succeed each other quickly, as in the common case, or slowly when they are made at longer intervals so as to shew the phenomena by single discharges—but in the latter case as well as in the former, the luminous and dark parts recur in the same places, being so to say localized.

6. In the dark part near the Negative wire, the strata of light often come into existence out of the obscurity and then disappear again. The dark part there is probably the same in nature as a dark interval between two luminous parts.

7. As much E. passes at the dark parts as at the luminous parts.

8. The light is not on the surface of the glass tube but in the space—in the medium within.

9. There must be medium there and this medium acts as a good conductor—as seen in the long thin termination, where it serves for a charging and discharging coating.

10. The medium there seems to owe its luminosity to its charge and immediately succeeding discharge. Where darkness occurs, probably the discharge more as an uniform than an intermitting effect.

11. The effect of light perhaps due to a series of charges and discharges at each luminous place.

12. The bands or luminous places are on the pos. side—or end—whether that be most or least insulated?

27 JANY. WEDNESDAY. At Royal Institution.

13. As the mercury rises, Mr Gassiot tells me, the column of strata becomes compressed, but the number of strata remain the same. This would seem to shew that as the medium was denser the strata were a smaller distance apart.

14. With one tube Mr Gassiot say[s] that the stra[ta] are not at

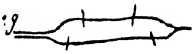
equal distances—being more numerous in an inch at one part than in another.

15. Weaker battery gives wider intervals of the strata?

16. The mercury rising always tends to cause disappearance of the strata. ? is it because the medium becomes denser.

17. Dispersion of the platinum is at the negative end only—the luminous column of strata proceed from the positive wire.

n:9



18. Tube of this kind—no interference?

19. Magnetic influence traced out by single pole of E. Magnet.

20. The pos. wire made if possible soft iron, end on, and made a magnet by external E. magnet.

21. Make outside ring coating P. and N. by common electricity or by Leyden Jar—make the P. and N. states intermit.

22. Examine light and dark parts by *sol. quinine* for rays—use lenses, etc.

23. Apply the Leyden jar so as to have a snapping current.

24. Fill a tube with steam—seal it and cool lower end: it ought to make a good vacuum.

25. The column of strata become by application of a magnet a real spiral?—revolving one way or the other.

30 JANY. 1858. At Mr. Gassiot's.

26. Employed Grove's battery, usually only 1 pr. of plates—to excite Gassiot's best Ruhmkorf A. Worked altogether with sealed Torricellian tubes.

N° 12. F

27. Tube No. 12 F. Barr. size, 0·4 of inch outside—2 feet long—wires platinum, 15 inches apart—top wire dips $\frac{1}{2}$ inch into the tube—bottom wire 7 inches from end. The discharge fills it with light—1 pr. of plates only and if platina plate diminished to $\frac{1}{4}$ its size or even $\frac{1}{8}$, still the discharge will fill it. The light is stratified—about 12 strata to the inch—they are concave to the Pos. wire and convex to the N. wire—they wander to and fro, shifting a little like northern corruscation, i.e. one into another. The P. wire has a bright glow on the discharge surface only and the stratified stream follows on directly from it. The N. wire has a red glow all over and around it and the dark space next to this, reaching to the luminous column—the dark space is about $\frac{1}{3}$ of an inch long.

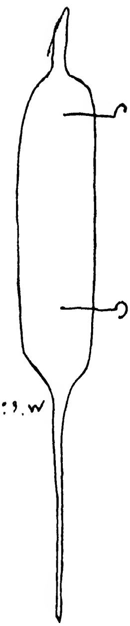
So narrow tube shews strata, and they are of one nature and successive from P. wire to dark space near the N. wire.

28. Tube No. 12 G–similar to the last but smaller bore–produces like results. The tail below the N. wire becomes luminous simultaneously with the upper light–and more luminous when the hand is applied as a coating against the outside there. A coating of tin foil did better–and still better if pressed close to the glass: if applied at the lowest 2 inches, it induced luminosity in the whole tail, yet most in the lower coated part. The intervals of the strata in the light column between the P. and N. wires were about as with tube 12 F.

29. Tube No. 12 H–like F and G but smaller still–is thermometer tube. Same column as before and the strata about the same number in an inch. There was light in the tail part as with tube G. Could perceive no strata in these parts either with this tube or G. So the strata occur in the finest tube and there must have been nearly 200 in the whole column.

30. The tubes 12 F and 12 H, of very different diameters, were put up in one series side by side, so that the same discharges might pass through both and allow of a comparison of the light columns as to the number of strata–they seemed to be very nearly alike in that respect.

31. A tube (Welch's, No. 3), one inch diameter–wires 8 inches apart. Excellent strata from the P. wire downwards about half way–here it required about 2½ to fill an inch–near the P. wire above, where the column commenced, they were more numerous. The strata split parallel to themselves and opened and travelled one into another, the dark and light parts shifting as if the determining circumstances were not constant. Hence they could not depend merely on the degree of rarefaction. Luminosity occurred in the tail termination below the N. wire, but it was feeble.



No. 3. W

32. Associated this (Welch) tube, No. 3 W, with the tube 12 H, so that the discharges passed through both in succession, to see if the number of strata was affected–but in each tube they kept nearly their former dimension. In this case the tail of the tube 12 H was luminous whether the wire immediately above it was P. or N.–but only faintly so when P.?

33. Tube No. 2 D was like 12 G in size, i.e. small barometer

size. The light was here apparently an uniformly luminous column and it was only by close observation that striæ were brought forth. On separating the lower wire from N. pole and leaving the upper wire P., the light fell much in intensity, but the whole tube was filled with it. There was then no current through the tube, only oscillation, and apparently no strata in the light. Must ascertain whether there is no strata in those cases where the current is not through but returns.

34. Hung tube 12 H up in the air by a silk thread, so as to have it free from outer conducting or coating effects, with the tail downwards and the Ruhmkorf terminal attached to it as before, i.e. the outer end to the upper platinum wire and none to the lower wire. Upper wire made P.–the tube luminous throughout, even to the end of the tail–decreasing in brilliancy from above downward–the presence of the insulated lower wire seemed to produce no effect–the brightness of a place seems in proportion to the quantity of charged surface beyond it, the electricity of which has to pass to and fro at that place. Found the alternations of light in this column, but the darker parts were very little less bright than the brighter–the number of alternations in an inch were about as before when the through current could exist, so that it does not essentially require the through current to produce them. There were no sensible strata in the tail, but still they were very weak in the other part.

35. Connected the lower platinum wire with the earth–the gas pipes–then the light column between the wires far brighter than before and the strata good and as before. If the lower platinum wire were recurrently insulated and touched by that from the earth–then the feeble or the brilliant column appeared. If the lower platinum wire were connected with an outer copper wire in the air, but with its end resting on glass–the light was much better than if that piece were away; but when the earth wire from the gas pipes touched this wire, the column was better still in brilliancy. Still, the effect did not rise up to that produced by connecting the lower platina with the inner end of the Ruhmkorf coil, for that is really N. to the current and not neutral only, and therefore discharges better than the earth.

36. When the lower platinum wire was connected with the earth,

there was the least light in the tail below it:—when it was insulated there was most light in the tail but least between the two wires. This agrees with the condition of charge, etc. of the parts.

37. When the lower wire was connected with the earth—we could not tell by the luminous appearance which was the P. and which the N. wire. Yet the strata were quite good throughout and the same from wire to wire. No red halo round the N. wire or dark space there.

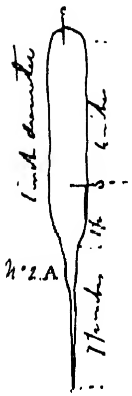
38. The secondary coil of the Ruhmkorf has of course an inner end near the core and an outer end outside—the inner is almost uninsulated whilst the outer is most insulated. The outer end is that which has been used thus far, and being the *inductric* to the earth connexion, can be made either P. or N. by the position of the handle or commutator of the primary coil.

39. Made the outer terminal of the Ruhmkorf connect with the top wire of the tube and connected the lower platinum of the tube with the earth—then rendered the inductric terminal either P. or N. but could see no difference in discharge at either end and could not therefore tell which way the current was passing. But when the lower platinum was connected with the inner or other end of the Ruhmkorf secondary wire, then could tell at once which way the discharge was passing by the glow state around the P. terminal and the dark space round the N. terminal. The P. terminal is not altered, but it is at the N. terminal that the change occurs, and this change occurs whether the N. terminal is in contact with the outer end of the Ruhmkorf, or with the inner end; i.e. if the upper platinum wire be connected with the outer end of Ruhmkorf and made N. inductrically, whilst the lower platinum wire is connected with the earth or with the other end of the Ruhmkorf secondary—the negative upper terminal is luminous as the P. or presents its own dark space accordingly; or if the wires of the Ruhmkorf be changed—the results are still the same.

40. Now associated the inner end of the Ruhmkorf with the upper platinum wire of the tube and left the lower platinum of the tube insulated. Whether the current from the Rhumkorf wire P. or N. arranged, there were no effects—no current thgh. the tube or impulse tending to charge it. Then connected the

lower platinum wire with the earth—there was then nothing with use of one or two cells—when ten cells on, then a feeble discharge now and then. So that all the important and luminous effect[s] are when the outer end of Ruhmkorf acts as the inductric. When that was put on to the lower platinum wire, after removing the earth contact, then all the phenomena recurred.

41. It will be important to remember this when considering the phenomena of the tube, for the two ends are not equal in P. and N. force. One is eminently inductric to the other. What will P. and N. discharge from a Leyden Jar do?



42. Tube No. 2 A hung up tail downwards. The outer Ruhmkorf wire to the top platinum and the inner Ruhmkorf to the bottom platinum. Always *one cell* of Grove's battery. The phenomena were good—but even with 9 cells there was no luminosity in the lower part or tail. Returned to *one cell* for the exact phenomena. Whether the upper or lower platinum wire was P. (the upper always being the inductric or outer wire of Ruhmkorf), the tube was about half filled with strata, from the P. wire to the middle distance; the rest was dark, except that the N. wire had its peculiar red glow—there was no effect in the tail except the Stokes' phenomena. Then changed the Ruhmkorf connexion, making the lower platinum communicate with its outer wire end, but the effects were the same. So this a true and complete case in which both ends of the Ruhmkorf secondary coil were influential and the earth's connexion cut off. With *two* cells and the lower platinum P., light appeared feebly in the tail.

43. Now connected the outer Ruhmkorf wire with the upper platinum wire, removed the lower one, leaving the tube so far insulated and incapable of a current through it, and used one cell. Light appeared at each Ruhmkorf alternation in *the whole of the tube*, and brighter in the lower tail part than in the wide part above. The upper part of the column near the charging wire *had the strata feebly* but large. Whether the wire was made inductric P. or N., the phenomena were the same.

44. Employed *two cells*, with the same result as to the strata and appearances, but the light generally brighter. Used *ten cells*—still the characters of the result the same except that the light was brighter. In this case, when the upper inductric wire was P.,

brushes of the usual kind went off from it into the air above. When N. there were no brushes—this is the ordinary effect.

45. So the tube is here charged by the [illegible] power of the rare medium within and discharged by *return* of the charge through the same course. There is no *through* current. But the strata are produced here. In this case there can be no interference properly so called.

46. The superior brightness of the tail part is due to the compression of the tube and therefore denser state of the electricity which goes to create induction outwards at that part. Whenever the current is compressed by any circumstance—its brightness increases in proportion.

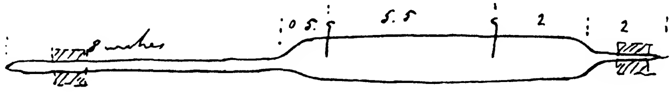
47. Now uninsulated the tube by connecting its lower platinum wire with the earth through the gas pipes—the phenomena immediately became the same as with the small tube (p. 8[1]), i.e. there was no dark discharge near either wire, whether the current goes up or down, with *one* cell or with *ten* of Grove's battery. The current must go through the *Ruhmkorf* to give the darkness near the N. wire. This looks as if obstruction had to do with the luminosity, since facilitation turns part of the light into obscurity. It suggests also that there is more obstruction at the P. wire onwards than at the N. wire onwards.

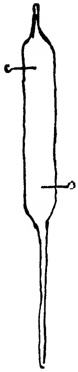
48*. Worked with another wide tube, No. 3 W,   inch diameter; this being held horizontally by two corks in a notched wooden stand and therefore *coated* and *uninsulated* there. When both the Ruhmkorf ends were connected with its wires, the discharge was a stream of light not filling the width of the tube and without strata—it was like the discharge in an air pump vacuum. The finger applied against one side of the tube caused the column to retreat towards the other side and become *brighter* at the part, as if the discharge were compressed into a smaller space. When the finger was moved in one direction round the glass, the column of light within moved round in the *opposite direction*, always keeping at the opposite end of the diameter joining the two. Would a coating round this tube form a dark ring or space there?

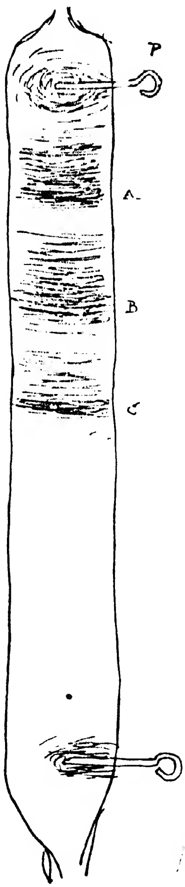
49. Another tube No. 5 (No. 2 E?). Is generally like tube 6, being an inch in diameter. It was suspended by silk: the upper

[1] Pars. 37–40.

* [48]







platinum wire connected with the outer end of Ruhmkorf and the lower to the inner end—the upper made P. The luminous strata were few, large and reached half way to the lower wire—then darkness and then the lower negative wire with its close red glow. When the knuckle was applied to the outside, the strata obeyed it in some degree, moving with it and being in part dependant on it—the knuckle acts like a coating. A *sound* was produced—feeble—but of a high pitch like a cricket note, but continuous and constant in pitch—it was loudest when the knuckles were applied to the middle part of the tube. Could then feel the tube trembling, especially if it were clipped lightly by the finger and thumb. When a slip of tin foil held in the fingers had one end applied in place of the knuckle, it performed the same office, and now found that it was *attracted* by the glass when close to it, *adhering* to it with some force. A second Ruhmkorf coil B had been used in this case, but now returned to the use of the first or former coil A and kept to it onwards.

50. With this A coil, and the outer end to the upper platinum and made P., the lower platinum being to the inner end and of course N., then the following was the order of the luminous appearances—at the P. wire P there was a glow, white; then darkness, which gradually rose up further on into a glow A, which ceased rather suddenly and then the darkness gradually rose up into another glow B—then sudden darkness rising up into a third glow C—after which darkness for three inches up to the glow on the N. wire. From the bottom or maximum of A to the like point of B was 1·25 inches. Great magnitude of the alternations.

51. Fingers affect these and their place. When the finger and thumb is applied at any place, the alternations settle in relation to them, and then if the fingers be moved up or down, the alternations move with them. If fingers carried up, the alternations go up, those that approach to the platinum P. disappearing into it and fresh ones appearing in the parts left below. If the fingers are carried down, the strata disappear below and flow into existence at the wire P. above. The place of the fingers is a [1] place.

52. When the fingers grasp the tube, it is felt to vibrate strongly—

[1] The words "dark? or light" have been written in pencil in this space.

and there is also a continuous high note produced. The vibrations or tremble occur in the lower and dark part as well as at the part above.

53. When an outer insulated Leyden Jar (pint) had its inside connected with the upper platina wire and its outside with the lower, there was no dark negative part; the light was continuous throughout and far brighter than before and the strata were scarcely apparent.

54. Now placed this tube No. 5 horizontal by means of the cork and stand, and associated the Ruhmkorf to both platinum wires. Looked for the brushes to the fingers at the side of the tube–they were there but not very good. Put a piece of tin foil as coating round the positive strata; it was about an inch wide and ring form, tied moderate close. When *insulated* it did not affect the position of the light and dark places, but when uninsulated, immediately the nearest light place went into it, and then if it was moved the light travelled with it. This tin foil gave a recurring pricking spark to the finger applied lightly to its edge.

55. Insulated this coating; then applied a little jar to take of[f] its charge–a weak charge could be obtained and that was P. by the gold leaf electrometer. If held long against the tin foil coating the charge did not rise higher. The charge seemed to be positive at all parts–near the negative wire as well as at the P. Whether the upper wire was P. or N., still the electricity gathered from the outside of the tube was P.

56. The long tube, No. 11, mounted horizontally and both wires connected with the Ruhmkorf. The strata were very beautiful, being about 18 inches of[f] the P. and 3 or 4 inches of dark space near the neg. wire. The hand applied to the tube felt the tremors and with peculiarities, for if held still they were hardly sensible, whereas if the hand moved along the tube to and fro they were strongly felt.

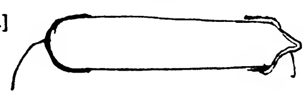
57. By adjustment of the contact breaker, strata could be made to appear in the darkness and associated with the negative wire–these were of a red colour–much fainter than the others and having a dark space, small, between them and the positive series–they had the reverse concavity to the former, being concave to the negative wire.

58. Curious and requires solution, that finger in p. 11[1] made the space *dark near* it, whilst in better tubes the light part came nearest to it.
59. Will a stronger current produce the effect of strata in a denser air? And if so, how far on towards brush in common air?
60. The number of strata in an inch appears to be affected by the diameter of the tube—perhaps by the vicinity of the glass and the state it assumes.
61. Will the tail end of the tube give glow when its wire, being the inductric, is made P. and not when made N? If any difference, what is it? Also if the tube be insulated and only the wire near the tail be connected with the outer Ruhmkorf, what will be the effect in the tail when the charge is P. and then N.?
62. The trembling of the tube shews its reciprocation with the discharge and I think by the medium. It appears to reciprocate to the pulsations within. The note produced is very high.
63. Try white of Egg on it. Also Lycopodium. Also the Kaleidophone.
64*. What would an induction charge only do? If anything, what would be the diffrence between raising the induction and lowering it? If light produced by the Ruhmkorf, the magnet ought not to deflect such a column. If by the Leyden Jar, then it ought to be deflected the two ways.

65. Observe the heat produced.
66. Try oxygen, hydrogen, Nitrogen and Carb. acid vacua—produced by the Barometer.
67. Try dark spaces for Stokes' effect.
68. It seems probable to me that convection occurs through a certain distance and causes approximation, and that then discharge of these accumulations occur to each other, with convection on the return of the particles from the discharge and so discharge again. By vibration of the whole column in subdivisions, these would quickly adjust to each other throughout the mass; but must try to ascertain where the discharge and the light are in relation to each other. One passage can give the alternating column, and it would seem to require that it should consist of innumerable elementary passages to account for the difference between these appearances and stationary undulations?

[1] Par. 48.

* [64]



69. Have fire balls any relation to these luminous phenomena?
70. Gassiot has sent me a note saying that wherever the clip coating were applied to the tubes—gold leaves connected with them repell each other. This accord with our always finding the outside positive, p. 13[1].

[Gassiot's letter, referred to in par. 70, is bound into the MS. at this point; it is printed below.]

30 Jany. 1858[2]
Saturday, 10 o'clock.

My dear Faraday,

After you left I made the following experiment*:

A No 5 Tube
b, b′ 2 Clips attached to
E a Dble. leaf Electroscope

Whether the clips *were as in figure* or *separated to their fullest extent* or *nearly in contact*, the leaves of the Electroscope always *repelled*, shewing that at no time were there signs of opposite states of Electricity on the Tube.

When Wheatstone saw the Experiment in this (or a similar tube), he considered the striæ as mere cases of induction, alternating + and −; if the experiment is correct this cannot be the case—at all events I give you the fact as I observed it,

Believe me
truly yours
J. P. Gassiot

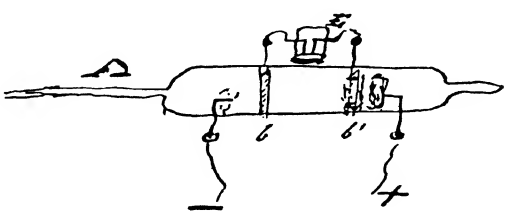
Dr. Faraday

WEDNESDAY 3 FEBY. 1858.

71*. The *long tube*, p. 13[3], No. 11. The wires are about 22 inches apart. That near the tail end has been always made neg. and has an extensive deposit of platinum wire there on the glass. The P. column of light strata is about 18 inches long and the N. obscurity about 4 inches. It was placed horizontally—is about 0·8 of an inch in diameter. A piece of tin foil put round the tube, so as to form a loose coating about 3 inches long, but open $\frac{1}{8}$ of inch at the top so as to see the state of the light column within. This put over the middle of the P. light column and then moved

[1] Par. 55. [2] Dated by Faraday. [3] Par. 56.

* [70]



† [71]

to and fro along the tube. The character of the column and its strata remd. generally the same but the strata seemed to move to and fro with the coating as if sticking to it. Leaving the coating insulated or touching it and uninsulating it made a little difference in the appearances and place of the strata. I think the strata were most regular where there was no coating–and next to that where the coating was insulated. The coating was applied only very loosely and not by paste or gum.

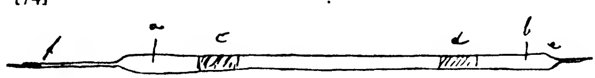
72. When this coating was brought along the column towards the N. wire, as soon as it reached the obscure part the latter was increased, i.e. the end of the P. luminous column was extinguished for about 2 inches. As the coating advanced towds. the N. wire, the end of the column retreated within it and the two could not be made to coincide. This effect took place only when the coating was uninsulated; when insulated the light was not extinguished.

73. When the coating was placed in like manner near the P. wire, it evidently increased the brightness and length of the end of the luminous column next the obscure N. space.

74*. *Inclosed current.* Coated the long tube in two places within the wires:–made these P. and N. by the Ruhmkorf, leaving the platina wires insulated–immediately the luminous stratified column appeared between the coatings with all the characters of the P. luminous column–but *no dark* part between it and the N. The stratification is as good as if the currents were *through*, yet here can be only *to and fro*, for the internal currents are insulated. The coatings were about 10 inches apart and each $2\frac{1}{2}$ inches long. As the size of the coatings were diminished the luminosity became less and less–but even when they were only $\frac{1}{2}$ an inch or less in extent the phenomenon was apparent. This is as it ought to be, for of course the current within is proportionate to the charged surfaces of glass at the two coatings.

75. No darkness at the N. surface. Well, there is nothing at this N. surface to make it–differs from the P. surface–all is glass, whereas at the *N. wire* surface there is a continual disintegration of the metal, producing a cloud of particles and an extensive lining of deposit. Is not this the cause of the obscurity? Would not a metallic lining within the tube produce the effect, and indeed, some important variations of it?

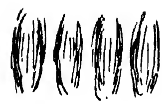
* [74]



76. In these experiments with inclosed currents, the further ends of the tube were luminous. The hands applied there increased the light—acting indeed as coatings.

77. Connecting the wire *b* with the earth—the luminous strata appeared well between it and the coating *d*. This was almost certainly due to a derived but *returning* current, not a real through current—for all current from *d* would have to return to it.

78. When this wire was to the earth by gas pipes, the luminous strata had a new appearance. Usually they are dished, the concavity being towards the P. wire, with the through current. With the inclosed currents before wire *b* was to the earth, this was generally the state; but when wire *b* to the earth, each luminous stratum became a very flattened spheroid, the outer surfaces of which were more luminous than the middle, as if there were a return set of strata consequent upon every direct set; the two effects being combined together and the intervening dark space becoming by that more restricted. Could these be opened out from each other by the revolving mirror, and any separate effect of the forwd. and backwd. current be established?



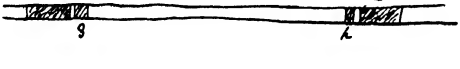
79. Put the coatings *c*, *d*, as far apart as might be without touching the wires *a*, *b*—the luminous column appeared as before.

80. Coated the extreme ends at *e* and *f*: the effect the same but smaller, because less charged surface.

81*. When two smaller coating[s] of tin foil, *g* and *h*, were near the chief coatings, small sparks continually passed between them and the large coatings; if the distance apart was a little greater, they were larger and fewer—if the distance still greater, they did not occur. In fact these became small subordinate coatings, which in contact with *c* and *d* acted the like part with them—but at a little distance could only take their part in the action by sparks from *c* and *d*—as in the diamond jar. But this only happened if the small coatings were *insulated*. If one or either were connected with the earth, then the passage of spark between ceased.

82. I suppose this is the action. Suppose *d* coating positive—the inner surface at that place will become negative (the vacuum within acting as general conductor there)—then the part within the small coating *h* will be negative inside and so require *h* outside to become positive—this it will do either by a spark from *d* or

* [81]



by a charge from the earth. As the state falls—all these effects will return.

83. If *g* and *h* (the small coatings) be connected together by wire, and so far from larger coatings *c*, *d*, as to give no sparks there, the luminous strata within are fine and reach from *c* to *d*: if nearer, so as to give a few large sparks to the great coating—also the strata. If still nearer, so as to give an abundant stream of small sparks—then the luminous column nearly uniform, i.e. unstratified. If both touch the large coatings, then the tube is dark, for in fact the Ruhmkorf currents are then all transmitted outside the tube.

84. Added *c* to *g* and *d* to *h* and took away the wire between *g* and *h*—so this is the first experiment with the outer coating and the effect was as before (   ). Connected each coating with the platina wire near it—then there was the simple and usual effect of a through discharge and the dark Negative space as on former occasions.

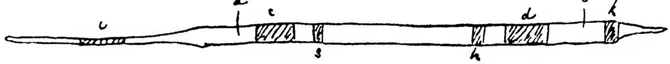
85. In this case, had again the clinging of tin foil slip to the outside of the tube as before (   ), but often lose this effect with shorter tube. Will it happen with the inclosed currents or discharges?

86. Gold leaf applied in slips at or near the outside of the tube with through discharge shews its Electrical state very well.

87*. If coating *c* and *d* be to the Ruhmkorf so as to have inclosed currents, and coating *g* and *h* be insulated (*d* being made Neg.), *g* and *h* do not affect the character of the discharge; but if they be *uninsulated*, they compress the luminous part of the discharge into their axis, and also render it more luminous there, as if the central part then conducted more power than before. This is the same effect as that of the finger in former cases (   ), but it seems to shew that the part of the medium near the finger is unfitted in some degree for the full act of conduction, and if so, by the facil obedience to induction, results which the uninsulated coating gives.

88. When coatings were added at *i* and *k* and these were connected with the Ruhmkorf, *i* being made neg.—then the inclosed currents reached from one to the other—and on touching either *c*, *g*, *h* or *d* to uninsulate them, the same compression of the light into their axes occurred as before.

* [87]



89. Now cleared off all the coatings and sent the Ruhmkorf currents through the tube, making *a* wire inductric and N.–it always having been kept N. up to this time and consequently the black coating of platinum being there within. Did not obtain the tremblings on the hand, whether still or moved ( )–or the high sound–or the attraction of the tin foil–or any crispation on white of Egg laid on the tube–or any effects with Lycopodium. Even a coating of tin foil round the middle of the tube shewed no signs of external charge or discharge. Yet all the phenomena inside of strata, etc.

90. But the inductric wire had been the N. all this time. Left the connexions unaltered, but reversed the current and so rendered *a* P. and *b* N.–and then the coating of foil gave plenty of outer electricity in sparks.

91. So changed the wires–made *b* wire inductric and P.–the *a* wire then becoming the inducteous and N. (least we should [ ] deposit of platinum at *a* inside ( )). Now obtained the song the brushes from the outer glass, feeble–and the tremors to the hand, etc. So that the inductric wire should be P. seems required for this set of results. So when *b* inductric and P.–outer signs, motion, spark, etc.; when *b* inductric and N.–only a little from the coating of foil–but poor.

92. The wire *b* in this tube has only been made N. a few times to-day by the through current, but the inner black coating of diffused platinum appeared in considerable proportion–it cannot be again dispersed or taken away.

93. This long tube, No. 11, associated with three large coatings of tin foil thus*, *c*, *m* and *d*–the wire *b* inductric and P.–the three coatings insulated. The little Leyden jar applied to coating *d*; sparks passed continually–if near enough could catch a fair charge by the electrometer sometimes, but only by accident–the recurring sparks shew discharge between the coating and the jar knob to and fro, and that natural. Same results at coating *c* and also at coating *m*.

94. Fixed a needle point on to the jar knob and avoided taking sparks. Now when opposite *d* coating, it collected a charge, always positive. At the middle coating *m* it collected a much feeble[r] charge, also positive–at *c* coating the charge was feeble

* [93]

and negative. In these experiments the internal current not through but to and fro.

95. Made the Ruhmkorf connexion to platina wires *a* and *b*, the wire *b* being inductric and the three coatings insulated. The wire *b* was rendered P. Now continued contact of the phial knob with coating *d* gave positive charge—but little or none was obtained from *c* or *m* coatings.

96. Instead of the jar, connected a gold leaf electrometer to coating *d*—the leaves diverged constantly, becoming N.; if discharged by a touch, they rose up again N. The same was the case with coating *c* and coating *m*. All this time the Ruhmkorf was acting, wire *b* being inductric and P. and wire *a* inducteous and N.

97. For a few moments reversed the inner current, making *b* inductric but N.—but the results were the same; all the coating[s] became N and if discharged resumed that state.

98. A small tin foil coating gave the same result. One of the brass clips gave the same result. Made the wire *b* P.—put a clip round the tube near it—but whether near to or gradually removed from it, it charged the Electrometer negatively.

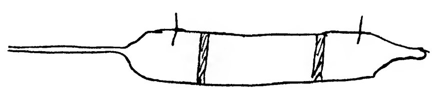
99. Much care is required in making the contacts in these feeble cases, for varnish or oxide of the apparatus often insulates and interferes.

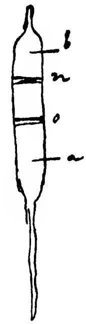
100*. Removed the long tube and took tube No. 5 (No. 2, E?) ( ), placing it horizontal and putting two loose brass clips on to it. Had a gold leaf electrometer of two separate leaves—connected one of these with one clip and the other leaf with the other clip—connected the Ruhmkorf with the wires of platinum. The bands were very good, but as to the gold leaves, sometimes they attracted each other and sometimes repelled each other and that irregularly, and whether they were near together or far apart, or over light or dark parts.

101. Striking—that these clips and their wires often took a charge which seemed permanent after the Ruhmkorf was stopped. Also that their motions were by sudden fits and starts, as if often caught by rapidly recurring and changing action during the Ruhmkorf action.

102. Took another of the shorter wide tubes, that called W,

* [100]





No. 3, and suspended it vertically in free air by silk thread. Made wire *b* inductric and wire *a* inducteous, so had the through current—*b* always P., and *a* N. Coating, being a ring of tin foil, *n*, to double leaf electrometer Λ; rendered it Pos. Made the lower wire *a* P. and the lower coating *o* connected with the Electrometer gave no signs—doubtful result. These uncertain results appeared for a while—at last, changed the tube and took another like it and took care to have all the contacts clean and good.

103. In this tube, made *b* inductric and *a* inducteous, connected both with the Ruhmkorf and had the two narrow coatings on as before—clips. Then connected the upper clip to the electrometer, and when the upper wire *b* was P. the Electrometer was P., and when the upper wire *b* was N. the Electrometer was N.—the clip outside taking the same character as the inductric wire near it.

104. Same position of things: top wire *b* made N.—then the top clip made its attached electrometer N. with difficulty. Top wire *b* made P.—then the top clip made its electrometer strongly but uncertainly P. With the top wire *b* made N.—the *bottom* clip to electrometer gave it a little N. charge. Top wire *b* made P.—bottom clip to electrometer made it much P. In this state, moved the clip from bottom gradually to the top, and it always made the electrometer P.

105. In this state moved the clip from top to bottom and it gave N.—clip below was N.—clip above was N.—again and again it was N. and well so—taken down to the bottom—all N.—again and again. All this while the upper wire was inductric and positive.

106. The Phenomena seem now quite regular and easy.

107. Also when the clip was either below the lower wire or above the upper wire, still it made the attached electrometer negative.

108. Now reversed the current, so that though the top wire is still inductric, it is also Negative. With the clip either above the top wire or below it, or at the middle, or above the bottom wire or below it, was always *negative* to its attached Electrometer.

109. *Stopped the Ruhmkorf action*—discharged the electrometer and its attached clip—left it a moment—both rose up into an electrified state, Negative—discharged it again—again it rose up—discharged it again—again it took a charge. Just as if the tube had

become charged P. on the inside and N. on the outside, and that as these gradually fell, so the outside shewed a gradual discharge of its negative electricity. Can the glass be charged like spermaceti, etc. a little way in and so gradually discharge? But whether the upper platina wire be made P. or N., the outside seems always N. Galvanometer gave no results.

110. Is not the negative obscurity dependant on the presence of the dispersed platinum there? It seems to me quite sufficient. There is no negative obscurity in the perfectly inclosed current. What negative obscurity does a large surface of mercury give—or the large surfaces in Plücker's apparatus?

111. Gassiot shewed me some of Plücker's tubes—but not the effects.

112. In reference to the action of the magnet on the inclosed current, see Gassiot's note and description.

[The following letter from Gassiot is bound in to the MS. after par. 112.]

Wednesday Evg.
3 Feby. 1858[1]

My Dear Faraday,

You left your pencil case on the Table, which I return. I am afraid you were a little fagged today.

I avail myself of the opportunity to enclose a note of two facts, both of which I will show you when we next meet.

If there are experiments which occur to you and which I can make, I will do my best. Never mind giving *reasons*, merely say try *so* and *so*, and I will then endeavour to vary the conditions, and not take any thing for granted

truly yours
J. P. Gassiot

M. Faraday Esq.

[Pars. 113–147 inclusive, which follow here in the MS., are not printed. They were written by Gassiot and numbered by Faraday, and are presumably the "note" referred to in the letter above. The experiments described were repeated by Faraday: see par. 148. References to Gassiot's paragraphs have been deleted from the matter which follows, but explanatory notes (in square brackets) have been inserted in the text where necessary.]

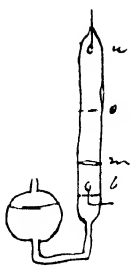
[1] Dated by Faraday.

148. At Mr Gassiot's–and he shewed me the phenomena described in his previous notes. First as to Electric state of the outside (112), repeated that experiment [with Torricellian vacuum tube standing in a jar on the plate of an air pump]–the upper wire to Ruhmkorf was Inductric and Pos., and the lower wire inducteous and Neg.–C and C′ [narrow outer coatings] were each one turn of wire–continued on to two gold leaves in a glass jar, insulated but acting towards each other. Whilst the mercury within was any where below C′, the leaves feebly repelled each other, shewing a general state of charge on the outside, and this had no reference to the luminous or dark strata. It was no doubt a general state induced by a general charge of the inside. As soon as the inner mercury was above the outer coating C′, the leaves attracted each other–shewing that the mercury discharge[d] the inside and consequently reduced the outside state and that the coating there, C′, then acted as earth contact.

149. It would appear that the state induced inside by the inductric Pos. charge above extends through the whole vacant space of the tube down to the mercury, or the Neg. surface.

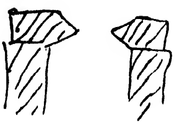
150. I do not think that the Electric state outside has any thing at all to do with the origin of the stratification or characters of the internal discharge–although it is evident by the action of coatings or the fingers that the latter can be affected by the state of the outside.

151. Put up the apparatus No. 6 with platinum balls [8 in. apart]–Ruhmkorf. Upper ball Inductric and Pos.–the lower ball N. When by the air pump the mercury in the tube was brought below the lower ball, then the stratified column of light reached from above far down towards *l*, which had the red glow–but when the mercury was allowed to rise and cover the lower ball, the strata retreated much and the obscure space between the P. and N. surfaces increased much. The appearance when the mercury was at *m* was thus: red glow $\frac{1}{30}$ of inch about upon the mercury–then a dark stratum about $\frac{1}{20}$ thick–then light stratum, becoming less luminous upward, so as to give the appearance of a lambent flame an inch or more in length which passed into the obscurity; after which came on the strata of light and obscurity belonging to the Pos. ball *u*.



152. When one pole of a cylinder Electro magnet is brought towards the tube–fine effects occur. Being made to traverse from above downwards, it affected the Pos. luminous column and increased its length, carrying it into the obscure part below–as it descended it affected the feeble lambent flame from the Neg. surface, driving it down an inch or more quelling as it were the phenomena of the Neg. pole and fostering those of the Pos. surface. If the mercury be allowed to rise to about *o*, then the Pos. column of light is very short, as the obscure and Neg. effects always exist, and rising up encroach upon it; but then the Mag. applied near the upper ball produced very fine strata much increased in brilliancy (211).

153. Now as to the Magnetic division of the double discharge, i.e. the enclosed current. Had a good horse shoe Electromagnet with its poles turned upwards and pointed terminations, so that the tubes could be placed between them and carried over and round either one or the other.

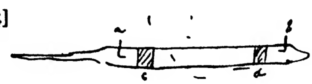


154*. Took the long tube No. 11 with its platinum wires *a* and *b* and also two tin foil coatings *c* and *d*. Connected the wires *a* and *b* with the Ruhmkorf–the light column well stratified appeared–placed it between the magnetic poles it then moved, trying to revolve round the poles, and wherever carried to, it moved tending to go as far as the glass would let it at right angles to the lines of magnetic force. It remained a single column or discharge and moved, as Mr Gassiot tells me, in the right direction according to the law formerly determined.

155. The discharge seemed much compressed against the side of the glass and much brightened, as if there was little or no discharge in the lateral dark part of the tube.

156. Now connected Ruhmkorf with the coatings *c* and *d* so as to have only the enclosed or double current–it was luminous throughout, there being nothing like negative obscurity. But there was a common *convexity* of all the strata and it was *from* the coating, which was rendered inductric and positive. This tube placed between the poles of the Electro magnet had beautiful division of the column into two with dark space between–as it was carried round the poles these columns seemed to rotate round each other, each in fact tending to obey the law by which it

* [154]



desired to travel across the lines of magnetic force. Each column had the convexity of the strata turned in opposite directions and it was thus easy to see which was positive in one direction and negative in the other. This magnetic analysis of the double stream is most beautiful. The strata were more numerous in the parts of the columns nearest to the Magnetic poles, as if they were restrained within a narrower channel. We shall see an analagous effect in the *Bonn tubes*—it seems to shew a dependance in part of the layers on the width of the channel which limits their extent. Each column rotated, as Mr Gassiot tells me, according to the proper law.

157. Surely the middle dark space here can carry no current or discharge—the two currents must alternate with each other.

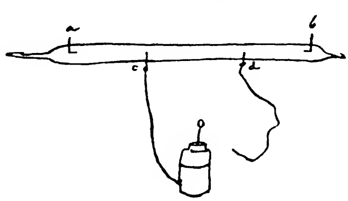
158. Does the Electricity in a stratified column move beyond the interval of two strata? Is not that the *unit of Electricity* contained in any one of the discharges?

159. When this tube (154) either with or without coating was held at one end with the hand, and the other applied to the Ruhmkorf, being merely laid on the coil, it became filled with a stratified column of light—and this being a double column divided by the close application of the magnetic pole. As it was carried nearer one pole or the other of the Ruhmkorf, so it was affected by the polarity of its core. When held to the conductor of an Electrical machine so as to take sparks or discharges on to a coating at the end, it was filled with light—but not examined for strata [Gassiot says he also observed a series of pulses of luminosity in the tube, becoming fainter and fainter, after it was removed from the conductor].

160. No. 16 or the Ether tube with the Machine gave light I think in the inside. Mr Gassiot reports it as not luminous—see also (167).

161*. The long tube No. 14—Leyden Jar charged Pos.—a wire from the outside to wire *a* of the tube—a wet thread fastened to wire *b*—then this wet thread brought towards the ball of the Leyden Jar; and at the first trial a very fine stratified column of light obtained. Repeating the trial, obtained the effect but not constantly. Occasionally a dark space could be observed near *a*

* [161]



but not constantly. With a shorter length, as that figured above[1], obtained columns of fine strata—convex from the Pos. or *b* wire, which was made inductric by the circumstances. Even when no wet thread was employed, but a wire from *b* brought near the jar knob so as to receive a brush from it—the strata were there and the convexity from the Positive.

162. Now the Jar was charged N. and of course its ball determined the inductric and negative condition upon the wire *b*. Found the discharge to be slow in many of the trials and difficult and with no strata. This slow discharge of the Neg. inductric remarkable. Believe that these trials were with the great distance.

163. Employed the middle wires *c* and *d*, which are about 10 inches apart, using the same arrangement of the Jar. When the inductric was Pos. there was a fine stratified column and dark space near the Neg. wire. When the inductric was Neg., there was the same good result and dark space near the negative—the convexity of the discs or strata were still from the Pos. or inducteous end. So the convexity and the darkness occurs even with Leyden discharges, whichever is the inductric end.

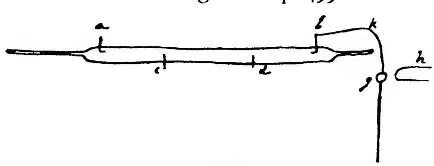
164*. A wire connected *a* with the earth—a wet thread *k* from *b* went to an insulated ball *g* which could receive sparks from the Pos. conductor *h* of the Electric machine. Such sparks gave light through the tube but no strata. Changed the thread *k* for a wire, and the ball *g* for the ball of an insulated Leyden jar—still the sparks only gave uniformly luminous discharge through the tube—then uninsulated the outside of the Leyden jar and now obtained the strata and a dark place near the negative wire *a*—the jar having always been positively charged.

165†. Mounted Tube 15 [3 ft. 3 in. long, mercury vacuum], which has no wires—applying coatings on the outside 36 inches apart. In this case there was light inside—but the effects were not regular—there was too much diffusion from the wire and coatings. The jar would receive only a very poor charge after many sparks had passed. The Electricity got away. The experiment with the tube without wires requires much care and preparation.

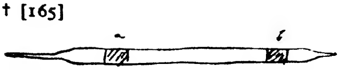
166‡. Returned to tube No. 14 (161): connected *a* to the earth

[1] In the diagram on p. 433.

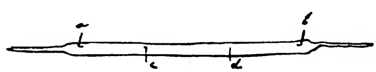
* [164]



† [165]



‡ [166]

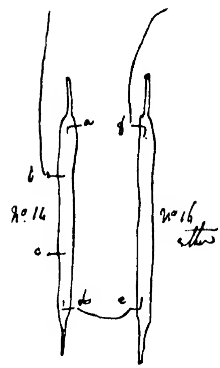


and *b* by a wire with the outside of an insulated jar. Sent machine sparks on to the ball of the Leyden Jar—there was light in the tube for each spark but not stratified. Made the connexion between *b* and the outside of the Jar by wet thread and then each spark gave the stratified discharge. The case is the same as by direct sparks from the conductor. But the effect of the wet thread and of the jar uninsulated all shews that *time* is wanted for the Stratified discharge.

167*. Experimented with the Ether tube No. 16 [3 ft. 3 in. long]. The Ruhmkorf was connected with wires *a* and *b*—there was no visible discharge through. Still, if the inductric wire was connected with *a*, and *b* with a gold leaf electrometer—the latter diverged, shewing that some electricity found its way through, for if the interval from *a* to *b* were air or solid glass, there was no divergence. Ether vapour therefore conducts some—only darkly.

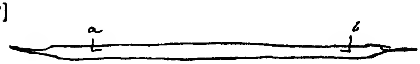
168. A water hammer, coated at *a* and *b*, having been suspended some little while, had these coating[s] connected with the Ruhmkorf. At first no internal discharge was visible, but after a time and with a stronger batter[y] the effect was produced, just as Mr. Gassiot describes it. Either the discharge was double or else—the current one way being visible—the return back was by the water on the glass. Probably the latter was the case and probably the visible discharge or charge was only a part of the whole—the rest going by the water on the glass.

169. The tube No. 14 and the ether tube No. 16 placed side by side, *d* and *e* connected together, *b* to the Ruhmkorf *inductric* and *f* to the Ruhmkorf inducteous. There were the results in No. 16 both above and below *b*; stratification, and that the more if the hand were placed as coating on the distant part of the glass—but there was no light or apparent discharge in the ether tube. Separating first *f*, and then *d* from *e*, there was no change, shewing that the ether tube did not enter into the circuit sensibly, but insulated. Applying the magnet to the streams both above and below *b* divided the currents and shewed them double. Putting on or taking off the ether tube made no difference in that respect.



170. But the two columns into which the magnet divided the general column were not equal; the one was far stronger than the other, probably twice as strong. This was due to the fact that of

* [167]



all that went downwards from *b* (which gave one luminous current under the magnetic influence) a part only returned (to give the other current), the rest escaping by the wires *c* and *d*. The same would happen on the other side and did happen, and it was beautiful to see how, as the Magnetic pole passed the inductric wire *b*, the larger of the resolved columns was below on the one side and above on the other; for as regarded the magnetic pole the two columns parting from *b* were in opposite directions and therefore tended to opposite directions round the pole.

171. [A glass rod had two coatings connected to the coil. When the coil was excited, a luminous discharge appeared in neighbouring tubes.] The supposed induction actions here were found to depend on the vicinity of the Ruhmkorf terminals to the tube shewing the luminous effect. In fact, a brush from a distant wire or a coating 3 or 4 inches from a tube is able to induce on its outside and produce light and even strata in it. When the tubes supposed to induce were taken away from their places, but the coating left, the effects occurred.

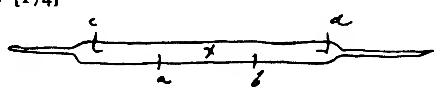
172*. The tube 15, with a coating on the middle and no wires, had the inductric of Ruhmkorf connected with it. It gave a beautiful purple light but no sensible striæ. There is a sound—and if the fingers be applied, this sound increases and a tremor is felt. The fingers at the side draw the discharge, i.e. the visible light, towards them. Both the streams divide by the magnet, but still shew no striæ or strata. The light is remarkable purple or red.

173. Long German glass tube No. 17 [5 ft. 3 in. long; wires 4 ft. 9 in. apart; mercury vacuum]. Ruhmkorf to its wires and the current through. It was full of fine strata all the way, with a little tendency to obscuration near the neg. wire. Whichever end was made N. gave a green phosphorescence (Stokes) on the glass there, whilst the other end was of the ordinary gray.

174†. Long tube No. 14 [3 ft. 3 in. long, mercury vacuum]—both *a* and *b* were connected to the inductric and made Pos. As expected, there was a neutral or dark place at *x*, then a stratified column each way from the wires *a* and *b*. The magnet applied shewed these as double columns and reversed them, etc. as was to be expected. All the appearances are as if it were the column of rarefied medium or ether that originated and sustained the

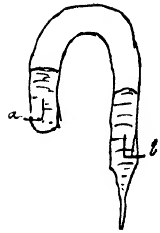
* [172]

† [174]

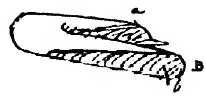


appearances, though they may be modified by circumstances of the tube itself or outside the tube.

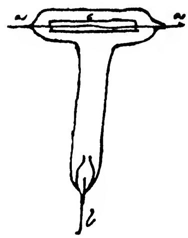
175. Apparatus No. 18 [mercury vacuum]. Ruhmkorf to the wires *a* and *b*—as these are covered with mercury, so both the P. and N. surfaces are mercury. Fine effect. Beautiful but rather close strata from the P. wire round the curve. There is a red glow on the N. mercury—then darkness—then large glow—then darkness and then Positive strata glow (151).



176. The discharge or passage at the P. surface is quite facil and goes off even from a small part of it—the discharge or passage at the N. surface seeks after the whole of it, and this is the case whichever is made inductric and inducteous. Inclined the tube so that the mercury in one leg was as in the figure. Well—when *a* was P. and *b* N., the luminous discharge filled every vacant part of the leg B and discharged upon *all* the exposed mercury in B; but when B was pos. and *a* neg., then the electricity left at the very end of the metal and all the space behind it over the mercury was quite obscure. So it is in other cases. In some of the tubes from Bonn the platinum terminals have this extended form. When made pos. the transition is at one or two of the nearest points on the metal, but if made Neg. the discharge falls on all parts of the platina and creeps round behind the edges in luminous discharge. Is this an effect of obstruction at the actual entrance into the metal, or is it an effect in the lines of force before they reach the metal? What would plumbago do as the Negative surface?

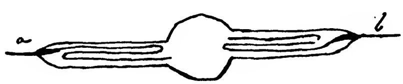


177. Now for some of the Bonn tubes. Very beautifully wrought—*a* a platinum wire supporting a brass cylinder to make surface large—*b* a wire, the inner end confined in a little glass cup within the tube—with Ruhmkorf to *a* and *b*, beautiful. The Pos. flame is red and the Negative glow purple. If *b* be positive, the luminous stream issues out of the little mouth and expands into striæ, exceedingly beautiful red, then the obscurity above and the N. purple glow.



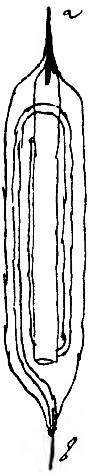
178*. A glass globe with two prolonged parts—each contains a fine tube beginning at one end, going up to the globe, returning and again going back to the globe, and *a* and *b* are platinum wires inserted into these tubes; so that the electric discharge has to

* [178]



enter one of these tubes, pass to and fro thrice, then across the globe, to and fro thrice at the other side and then out. Well, by the aid of Ruhmkorf, it does this beautifully. The discharge is white in colour—the fine tubes are full of close strata and these apparently seem to coincide in place in the different parts of the tube.

179. Another large tube—beautiful—colour of column very red, and this said to be produced by Alcohol.



180. Another. A wide tube—within it a moderate tube, beginning at one end and going to the other end, where it is open. The platinum *a* is fixed in the closed end. At the other end the platinum *b* is fixed in a fine tube which goes up the tube, returns at the top, descends, turns at the bottom and ascends again, so that it is open at the top. So tracing a discharge from *a* to *b*, it descends a moderate sized tube—then it ascend[s] in the capacity of the great tube enters the fine tube above—descends by it—turns ascends by it—turns again—descends by it—and terminates on the wire *b*. With the Ruhmkorf the whole of this is filled with a luminous discharge—which is brightest in the narrow tube, palest in the great tube and of medium brightness in the medium tube. In all parts the strata are seen. They are closest in the narrow tube—farther apart in the medium tube and widest in the great tube, and it is very interesting to see these large strata, pierced as they are by the smaller tubes, but keeping their position across the lines of discharge and their continuity across the tube from side to side. Of course the section of any one part is equivalent in power to the section of any other. The currents inside and outside the tubes do not appear at all to interfere with each other, which shews that the medium is the true seat of the phenomena.

181*. Another form—very beautiful.

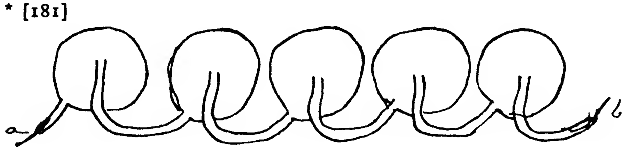
182. Dove on light of the Electrical discharge, brush, etc., etc. and the dark place—Phil. Mag., XIV, p. 386, etc. In connexion with the striated light:

Biot—light of the Electric discharge: Ann. de Chimie, LIII, p. 321[1].

Light references in Exp. Researches, 1381–99, 421, 2, 44, 51, 4, 5, 7, 61, 74, 5, 1544, 60, 687.

[1] This reference cannot be traced.

* [181]



183. Is not the luminous and stratified discharge a disruptive discharge, i.e. not a case of conduction–except as conduction and insulation with disruption are extreme cases of the same phenomena?

184. Is the stratified discharge a true but large expanded case of discharge of polarized particles to each other? Or is it rather a case of charge of the particles bodily in a certain group and then their discharge–as a brush may be supposed to be?

185. Silver–Iron–Tellurium–Red Phosphorus–Spermaceti–Wax–Ice–Glass Shellac–Sulphur–Gases dry–rarefied–vapour of mercury, etc. Is this a true series? Is the transmission of a charge by a torricellian vacuum the same as conduction in Silver, Tellurium, etc., or is it essential[ly] different from it?

186. Every action requiring *time* appears to be performed by means of a *medium* and is accompanied by a relation like that of Inductric and inducteous. The *time* in the Stratified light cases may be required in the charging of the particles (if they are charged), or for the act of transmission by the medium. Can the two be distinguished?

[Paragraphs 187 and 188 are Gassiot's.]

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187½. At Mr Gassiot's, when he gave me the MS. notes (187, 188).

Tube 13, 3 [A short mercury vacuum tube having a narrow end] had been dealt with as Mr Gassiot describes (187) [i.e. re-sealed after perforation by the spark]. Now with 1 cell of Grove to the Ruhmkorf and that connected with the tube wires above and below, it gave red at the negative and red flushes from the red strata above belonging to the Pos. inductric wire–these red flushes frequently piercing into the obscure space towards the Neg. When 5 cells were employed, the light, red, reached from one wire to the other, with very shifting stratification moving continually. I could not separate these distinctly by the moving mirror. The red colour is apparently due to the attenuation of the medium, i.e. to the degree of it in this case.

188½. The tube at the N. wire was highly fluorescent and the light of a greenish colour; it had a marked form on the glass, with sharpish edges, the forms stretching up and down like fans

on the glass—a magnet moved these forms; and the whole effect was [as] if luminous discharge was resident upon the glass or very near to. Yet on careful inspection had reason to doubt this, and believe that the whole effect was produced by the deposit of platinum at the Neg. wire acting as a mirror and sending back the rays on to the glass about, where it produced Stokes' effect in the form of *caustics*. The magnetic moving of these caustic[s] depended on the displacement of the light or source of the rays about the Neg. wire according to the natural law of magnetic displacement (p. 59[1]).

189. Examined the light and dark parts of a stratified column of light by two Nichol's prism[s]—found nothing particular in the results.

190*. No. 17 a long tube—covered with gold leaf and gum at *m*. Sent the Ruhmkorf currents through. Stratification very fine—it occurred within the gold leaf coating as well as outside. By clearing a line of gold away above and below, the stratification within could be well seen. The Magnet applied to this through discharge sent the whole current to one side as it ought to do. But when the inductric Ruhmkorf was connected with *a* and the inducteous with the coating and not with the wire *b*—then the current, excellently stratified, was within and double, and the magnet accordingly divided it.

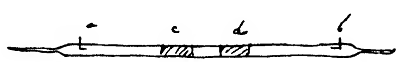
191. As to the effect of *stronger and weaker currents* in producing the stratification or altering it—a stronger current will often bring it on when a weaker does not shew it but only an uncertain luminous column. A stronger current brings up the brilliancy of weakly luminous stratification without altering their places or distances—until by increasing strength the successive stratification[s] seem intermingled and tend to produce a column luminous throughout, the dark spaces and effect becoming less and less distinct. By the Leyden Jar rise up to a luminous discharge all through.

192†. Took the ether tube No. 16 A (a second one, first broken), which does not give the luminous discharge through—put two tin foil coatings on at *c* and *d*—connected these with Ruhmkorf excited by one cell. When the space between *c* and *d* was about

[1] A pencilled reference; see par. 242.

* [190]

† [192]



3 inches, there were discharges within the tube. These appeared as brushes and sparks of different degrees of intensity passing along the inner surface of the glass and not visibly through the clear space or medium. There were no strata. These discharges must have been to and fro, but the magnet did not shew any arrangement on the one side and the other, whether one or ten cells were employed to excite the Ruhmkorf. The sparks did not go to and from the edges of the coatings, but over their surface, often to the inside of the coating, yet close to them–these must have had some dependance on the medium there–perhaps it became charged by the coating inside at the moment.

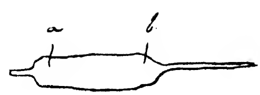
193. When the coating[s] were separated more and more, less of the discharge was effect, and [at] 6 inches distance there was but little of it. Occasionally sparks passed outside the tube between the coatings–but they were easily distinguished from those inside by the green colour of the latter.

194. Then raising the power from 1 to 10 cells did not produce stratification.

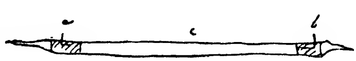
195*. Took a long tube giving strata by Ruhmkorf. Put coatings over the two wires, held one in the hand and brought the other near the prime conductor of the machine to take spark–each spark gave luminous column but no strata. The wet thread would have produced strata (161, 4). So obtain an effect here of intensity, and shews the *time* necessary to acquire the state, i.e. the medium state.

196. Connected *a* and *b* with the ground and rubbed with amalgam on silk at *c*–had luminous discharges–but no stratification. Had also a magnetic division of these discharges, for they were of necessity double–but not regularly–irregular as the discharges were.

197. Tube No. 13, 5 is a short thick tube: *a* made Pos. and inductric and *b* N. and inducteous. Is a good tube and yields good stratifications at the Pos. part. The finger applied at the side causes darkness against the side–applied all round causes darkness all round, but makes the light come to the middle, forming a luminous axis there. When the cylinder electromagnet, unexcited, was applied at the side, it acted as the finger. But when excited,



* [195]



it moved the light along the tube, drawing it out from the Pos. wire down the tube towards the Neg. wire.

198. When *a* was made Pos. inductric and *b* joined to the earth by gas pipes—then could not tell by the appearance of either the column or the wires which way the current went; both wires had a column of luminous discs separated by a dark space, each column had its convexity turned from its own wire, and when a pole of the cylinder Electro magnet was placed between them, it brought the two ends of the respective columns together, and that whether *a* was Pos. or Neg.

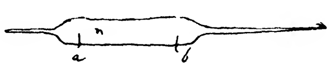
199. When the tube was thus* and *a* rendered inductric, the light cloud or stratum at *n* seemed to rise up and extend over the wire *a*, as if it were part of it; but on the whole I believe this to be reflexion of the light on a wire by the concave glass surface beneath on to the glass above, and there producing Stokes' effect. It move[d] by the approach of the Mag. pole and in the opposite direction to the motion of the light on the *a* wire. The effects were the same when *a* was made inductric Negative (241).

200. The discharge when *b* was to the inducteous Ruhmkorf wire or to the earth seemed to divide by the magnet, as if it were a double current—but is there a double current, or is it reflexion (199)? Experiment on this.

201. Coated the end beyond *b* and connected that with the earth. Made *a* Pos. or Neg., but the discharge was so like in character that I could not tell the difference.

202†. Now some curious results as to preparation or education of the wire or tubes. No. 20 is a new long tube with double wires (188). *a* made Pos. and inductric and *b* Neg. and inducteous: then a fine column and stratification produced or developed much on a former occasion (188)—but with *a'* Pos. and *b'* Neg. the column was luminous but not stratified. Continued the latter connexions and the action of the Ruhmkorf—by degrees the stratification came on and became very fine, the globes or strata of light running up and down the tube beautifully when affected and led by the magnetic pole.

203. Now made *b* inductric pos. and *a* inducteous Neg.—gave column of light throughout with very little appearance of strata, being the converse of *a* and *b* above. Returned to *b* and *a* con-

* [199]


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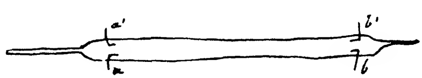


nexions: found that whether *a′* or *a* is connected with *b*, stratification occurs freely, but not if they are connected with *b′*. Seems like a case of education or preparation, as if *b′* made Neg. could not give the strata, and that *b′* had been raised by continuance of action to that state. For a moment moved the connexion from *b* to *b′*, so that *b′* and *a′* were in relation to the Ruhmkorf: at first made *b′* Neg. and *a′* Pos. and obtained fine strata, but on reversing the current and making *a′* Neg. and *b′* Pos. could obtain no strata.

204. Now made *b* neg. and *a* positive—some stratification, but on reversing the current there was a marked increase in the abundance. It does seem as if there were a difference dependant on the platinum wires more than on the medium which is common to all of them—as if the wire were a reed to the column.

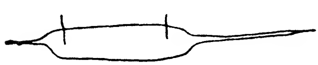
205. Went away for half an hour but these differences remained.

206*. Tube No.   , being a *hydrogen air pump vacuum*, called *Hy.* 1. Being attached to the Ruhmkorf, it gave a uniform column of light of a purple colour, not divisible apparently into striæ. The Negative dark space was there and about half an inch wide. The brush in hydrogen at common pressures or rarefied is, according to former Expts., Exp. Res. (1459), of a pale gray green colour. The effect of colour is a proof that the discharge is in the medium.

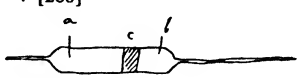
207. Tube No.   , being another hydrogen tube like that above and called *Hy.* 2. Has like qualities with the former—discharge of the same colour, i.e. reddish purple. Is the best vacuum of the two. The column is perfectly stratified—the strata are very close, there being twenty or thirty in an inch—the dark spaces are very dark and the stratification very regular. There is the usual negative dark space. The ordinary horse shoe magnet scarcely seemed to affect it. The great Electromagnet sends the column to one side, but does not press it up against the sides as in the Torricellian vacuums.

208†. Put a coating on to this tube at *c* and made *a* inductric and *c* inducteous—working for a double current. Obtained a column consisting of very fine minute strata. On applying the Electromagnet, separated the column into two, but the division appeared difficult and the one current was far stronger than the other. I suspect through discharge to a considerable extent—probably from *b* across the air to *c*.

* [206]

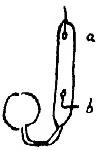


† [208]



209*. As to the conduction or discharge of these tube[s] for ordinary electricity, No. 11, being the long tube of the case, had tin foil put about *a* and *b*. Then *a* being in the hand, *b* was applied to the conductor of the machine. The conductor could give good spark before, but now it gave only small sparks indeed—the tube carrying off the electricity. There was a charge in the tube, doubtless on the inside, between the places of the wires, for when tube was held with *b* against the conductor—then lifted up—the prime conductor discharged—the tube put down again and the prime conductor again touched—it gave a spark.

210. The ether tube No. 16, applied in the same way, lowered the charge of the conductor. It also gave the residual spark from charge inside—once or twice it gave a spark through from wire to wire. There must have been dark conduction or discharge at times.



211. No. 6 tube, ball terminals. Repeated results of (152). Either pole of the cylinder electromagnet produced results, which were exactly alike except as to the direction of rotation of the whole column round it. Whether it was the upper or the lower part of the light column, they tended to go round the same magnetic pole in the same direction. The power of the magnet in drawing down the balls and strata of light from the upper Pos. inductric ball *a* was wonderful.

212. When the Mercury at *b* was below the ball and *b* negative, the electric discharge went round the ball and covered all the metal surface. When the mercury was half way over the ball, there was no sensible difference in the appearance of the platinum and mercury surface. As the mercury rose towards *a*, there was much deposit of that which rose in vapour on the glass, and by letting the fluid rise and then causing it to fall, it washed off this deposited surface and shewed the line of demarkation clearly. There is much vapour of mercury doubtless in the tube in this state.

213. As the Magnetic pole acting at *b* when mercury is above the ball reduces the Neg. flame or light there, it seems to do so by affecting and repressing that which hinders the elongation of the Pos. light, etc. from above; as the former light falls, the latter light comes down after it.

* [209] 

214. Put a coating on the part midway between the balls and made it Neg.—then there was a double current—well stratified—and divisible by the magnet.

215. A like apparatus, No. 1, but with pointed wires instead of balls, gave fine effects of the same kind—especially with 5 cells.

216. The tubes sing. This cannot be by the communication of vibrations from the Ruhmkorf because their spiral wires only connect them—long and covered by silk.

217. Cannot originate from the mere succession of discharge impulses, for these would give a very low note—unless each current caused several vibrations and thus made a note high enough to sing.

218. Originating *at* the tube, it may be a proof of the affection of the medium within it; reciprocating to the tube itself. I think it must have same reference to the medium as a column (of air), because of its dependance (in distance apart of the strata) in some degree upon the diameter of the tube or size of the space. Also because of its state in the complicated tube (180).

219. By (202), the platinum wires would seem to act as a reed in setting a vibrating column going.

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220. At R. Institution. Expts. on *mere conduction* of the tube vacua, for which purpose employed the large gold leaf electrometer, charging it by friction with warm flannel. The leaves can diverge until the extremities are 6 inches or even more apart. The instrument holds charges equivalent to 2 inches of divergence very well and for some time together.

221. Tube 15 A is 3 feet long and 1·2 wide and is without wires. Holding one end in the hand and applying the other end to the charged Electrometer cap, it brought down the divergence from two inches to one inch. Again excited the Electrometer and applied the tube; it brought it down even more, but kept some divergence. Repeating the experiment and exciting the Electrometer more at the beginning, the application of the tube would cause a sudden fall to a lesser divergence, and after that, raising the tube would cause the leaves *to open*, as if the inside had assumed a contrary state and were acting by induction—that contrary state

coming on by a previous sudden brush or equivalent discharge within, equivalent to *disruptive discharge.*

222. When the leaves were open and the rod end in contact with the Electrometer–applying the hand as coating on the upper surface at that part caused the leaves to close much–then taking away the hand and the rod caused them to open again, as if there had been disruptive discharge from one side to the other within the tube.

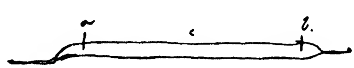
223. There were no signs of true conduction through the medium or the glass–but whether, when continuing to excite the Electrometer by the flannel and producing continual sinkings by the continued contact of the tube did not depend in part upon the glass (outside), is uncertain.

224*. Tube 23 is three feet long, 1·2 wide, has two wires and consists of a hydrogen vacuum. The wire *a* was held in the hand and the wire *b* brought to the cap of the excited Electrometer (   ); the divergence fell, but not entirely–a small charge remained. Recharged the Electrometer, reapplied the tube wire and again had a fall–repeated this–a certain feeble charge was insulated–if a higher charge was given, the excess passed away.

225. Held the tube by the middle as at *c*–applied *b* wire to the excited Electrometer; the divergence fell at once to 2 inches and would go on falling slowly to about 1 inch if the contact continued. Holding it at *c* and applying either *a* or *b* to the uncharged Electrometer, there was no divergence. Then charging the Electrometer well–touching it by wire *b* lowered it–quickly separating the tube–discharging the electrometer and applying either *a* or *b*, charged it up again. So it is evident the inside becomes charged and delivers the charge up at either end–but I do not think this is by a simple act of conduction of the space or medium, except as all discharge may be conduction in a manner.

226. Made this tube 23 warm and then repeated the experiments: the results were the same. When the wire *a* was in the hand and the wire *b* on the Electrometer cap, and the excitement of the latter gradually raised by the flannel–the divergence rose to a certain height and then went down suddenly, about half way. As the excitation continued, this happened again and again, as if when

* [224]



the electricity was at a certain tension, there was a disruption discharge through the medium from end to end.

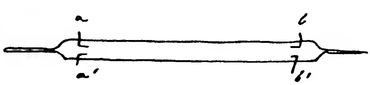
227*. Tube No. 20—same length and . The intervals between the wires *a a′* and *b b′* are about $\frac{1}{3}$ of an inch—*b b′* had been made negative and had much platinum diffused on the glass within—*a a′* wire quite clear. Holding *b b′* in the hand and approaching *a* or *a′* to contact with the charged gold leaf electrometer, a very large divergence was sustained = to $2\frac{1}{2}$ or 3 inches; applying a little more excitement, it would rise and then suddenly fall to 2 inches or thereabouts. When the tube was held by the middle, the same thing occurred. When the tube was held by the middle, touching *a* with a finger and bringing *a′* on to the cap of the excited electrometer, the charge would either fall at once or perhaps after a moment, leaving a divergence of about 2 inches; shewing that the greater charge fell by a sudden or brush or disruptive discharge, not through conduction by the medium, and also that the interval of $\frac{1}{3}$ of an inch was able to sustain 2 inches of divergence. Good.

228. The same was the case at the *b* end, except that the coating of platinum appeared to act in reducing the divergence to less than 2 inches at least, the insulated divergence was less.

229†. No. 11 tube—long—in the case—a wire at each end. Held *a* wire in the hand, brought the glass at *m* into contact with the cap of the excited electrometer: the insulation was very good with $2\frac{1}{2}$ inches divergence. Turned the tube on its axis so as to bring the wire *b* into contact with the Electrometer—then a sudden fall of the divergence to 1 or $1\frac{1}{2}$ inches, at which it stood. This effect was due to a discharge at the inside from the wire *b*, and shews that with the wire in contact afterwards there is still a certain amount of insulation. There seemed to be no sudden discharge through to *a*—only a charge of the inside surface of the tube and consequent slow discharge afterwards.

230. Last night at the R.I. By inclining a new syphon mercury apparatus, the prolongation of the mercury at one end could be extended to 8 inches. When this, being associated with the Ruhmkorf, was made the Neg.—the light filled the space above to the very end, hovering over the mercury and shewing that the whole conducted. Also there were abundant stratifica-

* [227]

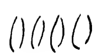


† [229]

tion[s] over the mercury, and so to say in contact with it, in that part of the discharge passing on to the further part.

231. The quantity of medium included in *one interval* in the tube No. (180) from Bonn could not be the same in the narrow as in the wide parts, the medium in all parts being at the same tension–it therefore could not be the seat or the measure of a definite charge. If there was any relation to an equivalent discharge through the whole tube, it could only be to a part of that discharge.

232. If a vibration is set up in the medium, then it may be *this part* of the discharge–an accident in fact and not measurable in the amount of the effect.



233. The coincidence in part (and its constancy) of the curvatures in the double current ( ) shews a dependance on the medium and its state of charge.

234. What does the curvature and the sharper definition on one side of the light strata imply? Combine it with the effects in the cone form. Is it not a result of the inductric domination? Make N. inductric.

235. Is the convexity never from the Neg.? If it is sometimes, what are the cases and causes?

236. With certain poles ( ), the positive and negative column cannot be distinguished in form, colour, or any other point, except their place in relation to the P. and N. surfaces and their magnetic rotation.

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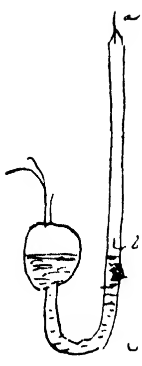


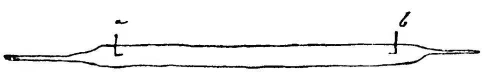
237. At Mr Gassiot's–he has an apparatus of this kind connected with the air pump and filled carefully with Mercury–the distance from *a* to *b* may be perhaps 18 inches–from *a* to *c* 28 inches. The pump easily brings the mercury below *b*. When the air pressure is let on, the mercury rises, but have a small cushion of air between the metal and the end of the tube at *a*–the wire at *a* wets with the mercury but still there is evidently a little air there. Now bringing the mercury down to *b*, putting on the Ruhmkorf and establishing the currents, and then letting the mercury very slowly ascend, the effect of making the vacuum less and less perfect would be perceptible.

238. So brought the mercury just above wire *b*—made *a* Pos. and inductric—the effects very fine. The N. mercury was rose colour at the surface—then a narrow dark stratum succeeded—then a white luminous stratum passing upwards into rose and the negative obscurity, and then the series of light and dark strata constituting what we call the Pos. column. As the mercury rose, the Negative light and strata and obscurity rose with it, keeping their relative dimensions—but the Pos. column became *shorter*. Also as the mercury rose and therefore as the medium became shorter and also more dense, the strata of the Pos. column *became closer*. By pressing on the contact maker of the primary coil, the discharges are made more sudden and intense, and that made the positive strata redder in colour, brighter, and also gave a conical form to the strata; so that they dipped into each other in a very beautiful manner, yet keeping their separate existence. One can hardly resist the idea of a propulsive axial force here; but remembering that at each discharge the inside of the glass tube is charged Pos., it may be that this act of charge against the glass retards the place of charge of the medium more there than in the middle. I think this is the probable cause of both the dished and the cone form of strata.

239*. Mr Gassiot has received from Cazelli six new tubes numbered from 24 to 29. They are all of one length and size, being from shoulder to shoulder 35 inches and from wire to wire about 31 inches. Most of them are an inch in diameter and some a little more. All were filled first with *hydrogen*—and then exhausted by the Torricellian vacuum process. They have not been essayed and are now submitted to the Ruhmkorf and examined for the first time. The tubes were rendered very dry and clean at the time of preparation and seem unexceptionable. Mean always to make *a* end Pos. and inductric.

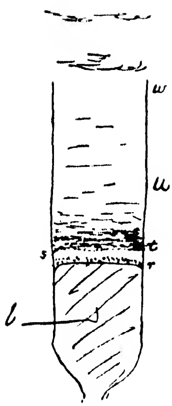
240†. The tube 26 (hydrogen) had tin foil coating on at *c* and *d* to allow of production of included current—connected *c* with the Pos. inductric wire of Ruhmkorf and *d* with the neg. wire—used one cell of Grove—the coatings were 22 inches apart. Ruhmkorf action for a moment but no appearance of discharge. Placed the coatings only one inch apart—still no appearance—and by the character of the spark between the Ruhmkorf terminals near the

* [239]



† [240]

coil, it seemed as if the tube were an insulation and did not disturb or take off part of the discharge force. But after three or four trials, discharge through the tube came on. The light was white and stratified, the strata being large so that the intervals were an inch or more apart. Increased the interval between the coatings to 22 inches–still the discharge through occurred–the whole distance was luminous but the stratification appeared as maxima and minima of light–and the recurring places were about 1·3 inches apart–the positions of the maxima were not fixed but shifted to and fro. This was of course an *included* current and the magnet divided it as before into two parts.



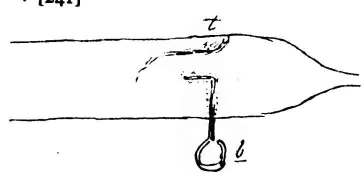
241*. Now *c* and *d* coatings were unconnected with Ruhmkorf and the wire *a* made *Pos.* and inductric and the wire *b Neg.* and inducteous. At first the tube was luminous throughout but without stratification–but *very soon* strata appeared–fine, large and improving in distinctness. When this had attained to a well developed state, the column of Positive strata reached to within 4 inches of the wire *b*, then there was the great obscurity, gradually passing into a weak light, another obscurity and after this the glow which settles directly on the *Neg. wire*. With a certain force on the contact breaker there was the tongue of light (199) above the Neg. wire, as at *t*†. This was not reflexion, but is I believe the beginning of the second light layer counting from the Negative wire. Now the order of the Negative light over mercury (or platinum) was this (238)–a low red glow on the metal which I will call *r*–a narrow dark space next to it to be called *s*–then a white bright layer to be called *t*–gradually becoming less and less luminous upward and forming that flame *u*, sometimes rosy in tint, which passes into the great negative obscurity *w* terminated by the occurrence of the Positive strata. Now I think that the tongue shaped *t* above is the *t* of the lower figure[1], the space between the tongue and the glow on the wire being the obscure space *s* of the lower figure. In many experiments with the air vacua, and without mercury, *r*, *s* and *t* all wrap round the negative platinum wire, and yet I think are seen in profile distinct from each other.

242. The caustics before described (188½) as occurring at the *b*

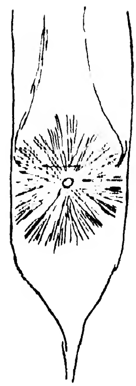
[1] In margin.

* [241]

† [241]



wire when Neg., appeared here at the same wire, and though they improved in brightness for a few seconds at the beginning, yet they evidently did not depend on the deposition of platinum there. On looking down on to the neg. wire from above, this fluorescent light, evidently due to Stokes' rays, etc. appeared around the wire and it had in it several dark spaces which I cannot help thinking are the shadows of the wire itself; i.e., I believe that it is the glow directly on the neg. wire which, sending forth the rays, many of these are reflected by the surrounding glass surface within which, being irregular because of the fusion of the platinum wire there, acts like a series of concave mirrors and sends the rays back to the other side, where such as pass into the glass produce light. The shadows of the wire are supposed to be from rays reflected then from the opposite side. The other more diffuse light, or rather larger light, and which always appears on the side and upper part of the glass toward the Pos. column of light, appears to me to be from reflexions taking place at the hinder shouldered part of the glass. If a glass cylinder be held vertical and a taper flame be held in the axis no caustics will appear; if the flame be moved a very little from the axis a caustic will appear on the opposite side—a very little movement in the flame will cause the caustic to move much. So a little movement of the glow on the neg. wire by a magnet causes these fluorescent caustics to change their place as if they moved directly under the magnetic influence.



243. Whilst the action was going on, the outside of this tube was strongly electric to tin foil. If close, it continually attracted it by a recurring action dependant on the charges. If kept a little way apart, there were continual sparks to and fro; but there was no charge of one kind—it is a recurring state due to the recurring charge of the inside. When the tin foil was held at a little distance, the successive sparks could be felt in the fingers as a prickling sensation.

244. After these experiments were over, examined the *neg. wire b* as to the state of the platinum. The wire itself, at first bright, had become blue by the action and much had been dispersed and deposited upon the glass surface around it. The electric struggle must be very strong at the negative wire. Yet all this time the

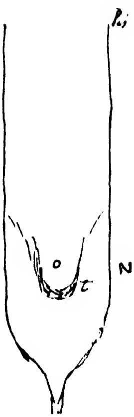
platinum had been Negative only inducteously and the Ruhmkorf urged by but one pair of plates. The Pos. platinum was perfectly bright and clean.

245*. Dismissed the last tube and proceeded to examine Tube 24, which has never been used and is the same, in size, wires, hydrogen vacuum and other points as the former ( ). Began by sending the Ruhmkorf discharge from wire to wire—*a* Pos. and inductric and *b* Neg. and inducteous—the small coatings *e* and *f* being connected with gold leaves in the same jar. The discharge through the tube came on at the first, but the light was equal, not stratified. The gold leaves shewed electricity, but both were alike and repelled each other.

246. Then the Ruhmkorf wires were removed from *a* to coating *c* and from *b* to coating *d*. The included discharge thus obtained at once was white and with places of maximum brightness, indicating the coming on of stratification.

247. Returned the Ruhmkorf wires from *c* to *a* and from *d* to *b*. The discharge was of uniform brightness, but being continued the stratification soon came on with large divisions.

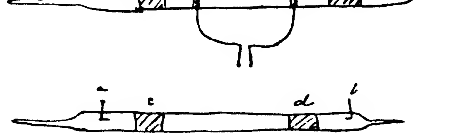
248. The tongue *t* appeared (241) over the wire *b*, the latter being turned downwards. It travelled well by the magnet, even the very nose of it behind the wire travelling round the tube as the magnet moved beneath it (of course on the opposite side of the tube). When the tube was turned half round on its axis, so that the tongue *t* was below, i.e. against the lower side of the tube, and a cylinder electromagnet placed with its S pole *under* the tongue but unexcited—then making the magnet active sent the tongue *to the right*—is this as it should be?



249. The fluorescence appeared just as before and from the first (242). The tin foil outside gave sparks as before ( ). The whole examination was not long continued, but still platinum had begun to deposit at the wire *b* on the glass there.

250†. Now took another of the hydrogen tubes, No. 25—it has a larger diameter than the other and is thought to be specially good, but has not yet been tried. The Ruhmkorf Pos. wire to coating *c* and the neg. inducteous wire to the coating *d*. At first no visible effects; then in two or three seconds the stratified luminous discharge and cracking noise at *d* coating to the wire,

* [245]



† [250]

shewing a sudden discharge to and fro—not a slow conduction. The discharge was of course included.

251. Now connected the Ruhmkorf Pos. inductric wire to the wire *a* and the Negative to the wire *b*. The strata did not appear at first, but a generally luminous column, and then the strata rose up well. The *end* of the neg. wire *b* appeared ruby coloured at the tip at first and the fluorescence also appeared at once, but the ruby glow spread over the wire shortly and the fluorescence increased. The magnet affected the place of the fluorescent caustics; and though it looked sharp, as if it formed on the glass the edges of a plane of separation between the electric phenomena, still I think it is only a result of the motion of the origin of the rays—the light on the negative wire. The tongue *t* (241) was over the neg. wire *b*; it and the other appearances all moved at once by the magnet.

252. We made the wire *b* the *inductric* wire, but still kept it *negative*. All the phenomena were the same. There was the same long stratified Pos. column—the fluorescence at the Neg. wire—the action of the magnet—just as before—so that making the Pos. wire inductric does not determine the essential phenomena.

253. Even the short moderate use of this tube had caused the dispersion and deposition of Platinum at the neg. wire.

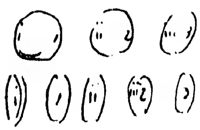
254. *Tube No.* 27—hydrogen and the same as the others. Connected the Ruhmkorf to the wires only—no coatings on. The luminous column, stratified, appeared at once but the character of the stratification improved for a time. Fluorescence at the neg. wire as before, and believe it to be reflexion which determines the form. The platinum was dispersed at the neg. wire and this dispersion in hydrogen shews it is without oxidation. The positive wire was a little dull before beginning and this dull state was not removed nor changed.

255. Mr Gassiot tells me that when the *Pos.* wire is wet with Mercury or has a globule adhering to it, this mercury is not dissipated by the action.

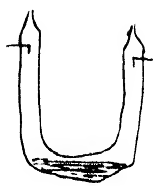
256. *Tube No.* 28 examined. Just as the former—the Ruhmkorf wires were connected with the tube wires *a* and *b*. The stratified column of light—the negative fluorescence—the tongue *t*—and all the other effects appeared at once.

257. *Tube No.* 29, hydrogen as the others–supposed to be the best. Ruhmkorf to the wires. Discharge at once–the light and colour uniform throughout–in a few seconds the stratifications, etc. as in the former cases. Phenomena just the same.

258. No. 13,3 is a 12 inch tube–that indeed which had been cracked and let down on a former occasion ( ) until it gave a red light. Connected with the Ruhmkorf–the Pos. light was in three or four balls–but by pressure on the breaker, these balls could be made to separate into strata, the three becoming 5 or so on, and these returning back again into the three–and all this by a motion of the smaller strata in opposite directions.

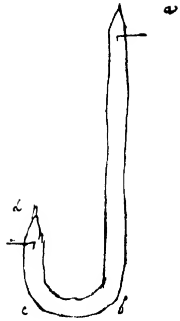


259. No. 13,5 is a like tube which at present gives white light and the balls, as was ascertained by connexion with the Ruhmkorf. The negative end of this was held to the Electrical machine, which quickly caused a spark to pass from the extremity of the vacuum, *cracking* the glass in doing so. Then it was connected with the Ruhmkorf and the action continued, and the change watched as the air entered. The colour of the light gradually became red. After a time–the balls of light subdivided as above by pressure on the contact breaker and joined again by relaxation. Ten cells on the Ruhmkorf gave the columns of light at *both* wires, the Pos. and the Neg., the strata of each being convex respectively to the middle space. The Negative stratification was very steady–but the Positive stratification was quivering. The negative obscurity *w* (241) was apparently between them.



260. No. 18 is the short syphon tube (175). Placed with the bend downwards and the wire ends upwards–the wires were connected with the Ruhmkorf and the mercury left unconnected with either but in the middle of the passage. The discharge was well stratified and passed all the way through the space over the mercury, being stratified there as well as elsewhere–a dark narrow space occurred between them and the mercury, but there was a bright reflection of them in the face of the mercury. The column was narrower but brighter over the mercury than in the fully open tube, as if the discharge were compressed there–little or none apparently going through the metal.

261. Now wrought with the long syphon tube–its length from *a* to *b* is about 24 inches–from *c* to *b* its width is about 5 inches–



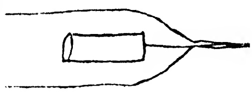
and from *c* to *d* about 6 inches. The quantity of mercury in it will occupy about $2\frac{1}{2}$ inches in length of the full tube, i.e. of an inch in diameter. The tube was laid down* so that the mercury lay in one portion, elongated, from *m* to *n*, 18 inches in length, the end *m* being 4 inches from the wire *a* and the end *n* 8 inches from the wire *b*. Only one cell was used with the Ruhmkorf; *a* wire was made Pos. and inductric and *b* Neg. and inducteous. There were positive strata from *a* through the whole space of the tube and over all the mercury up to nearly *b*. Some of the electricity entered the mercury all the way along, for the brightness of the column above the mercury became less and less up to *n* and then a discharge of Pos. electricity left the mercury there, manifest by its light, and brightened the rest of the column on towards *b*–this addition ran into the strata going from over the mercury, not disturbing them or making them quiver but brightening the whole discharge. Judging by the appearance, I do not think that half the electricity entered the mercury.

262. This result is very striking when collated with the other results with mercury electrodes (230).

263. When by coating the discharge in this apparatus was made double, the magnet divided it over the mercury.

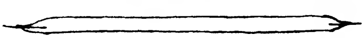
264. Now passed to the examination of certain tubes from Bonn. One, No. 2, was 39 inches long, $1\frac{1}{2}$ in diameter†, and had been filled with hydrogen. When connected with the Ruhmkorf, it gave a luminous discharge of a purple colour–striated or stratified–but the strata close–imperfect–not always reaching across, and shifting in place.

265. Another German tube from Bonn, to be called No. 1, was of the same size but had last contained air. It[s] negative electrode was an open brass cylinder attached to the platinum wire. The discharge was luminous–of a purple colour. The strata very close. The negative obscurity was there. The negative electrode was covered with light both inside and outside. There was first the light directly in contact with the metal–then next that the layer of darkness–and then the layer (   ) of light passing into the general negative obscurity. Even the inside of the brass cylinder had as its core this last obscurity, and between it and the metal the two layers of light and one of darkness. In that respect



* [261]

† [264]



it coincides with the mercury results before described (260, 261).

266. Another Bonn tube, No. 24, was 39 inches long, about $\frac{1}{3}$ or $\frac{1}{4}$ of an inch in diameter (Query fluoboric gas). It was full of fine small strata with deep blackness between. These were continued to within an inch of the negative wire and were often beautifully conical throughout. I think the form depends on a condition of the glass. The colour was purple white. There was a fine green fluorescence.

267*. *Bonn tube No.* 7. Contains Fluo silicon vacuum. Stratification every where and always perpendicular or nearly so to the lines of electric force. There were very fine conical strata at the passage from *a* to the larger space and also *b*, *c* and *d*. The light was most brilliant in the finest tube *a c*.

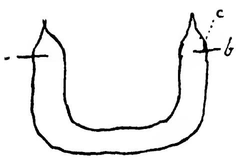
268. *Bonn tube No.* 6. Same form as the last. The same phenomena. The colour generally of the light is redder.

269†. Bonn tube No. 21. This tube is said to contain the vapour of anhydrous Sulphuric acid. The appearance was very beautiful—the strata very fine. The conical strata were exceedingly beautiful, especially where the Pos. discharge passed from the small tube into wider space. In the spindle shaped part there was a remarkable appearance, as of clouds travelling among the strata, not quickly but slowly, perhaps an inch in a second. Pulling on the action, they would go one way and then soon cease and not reappear. Reversing the current, they would then go the other way and then again cease—the strata continuing all the time—then reversing the current they would appear again and so on. They suggest some phenomena of convection as going on with the action.

270. The tube 13,5 (259) which had been let down and then sealed up by the blowpipe was again examined and found to retain its state as it had been left.

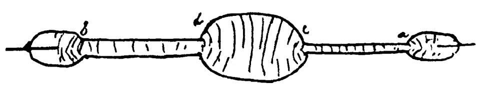
271. Is there not some indication that the strata are determined by some *pre-electrification* of the glass?

18 MARCH 1858. Mr Gassiot here.



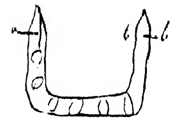
272. *Royal Institution.* Mr Caselli has made two tubes of this kind—of tube 1 inch in diameter. They are mercurial vacua from atmospheric air. The shorter one, No. 31, tried for the first time

* [267]

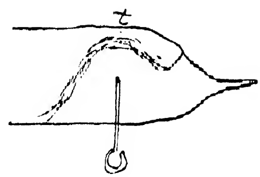


† [269]

by Mr Gassiot and his Ruhmkorf. The light passed at once—was uniform in tint. At the Pos. inductric *a* there was a fine glow, brush or cone opening into the full stream of light—but there was no light on the Neg. *b* wire, only a small spark at *c* in the angle of the wire and the glass on to which the luminous column discharged itself. I think all the electricity or nearly all passed into this spark, very little, if any, being taken up by the part of the wire projecting inwards. By degrees the appearances improved: a reddish glow came over the *b* Neg. wire within and the stratification gradually appeared, but the maxima of light were large and about six in number. A feeble Ruhmkorf power was best. The tongue *t* (241, 248) became very good.



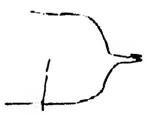
273. Tube No. 32 is the same in form but a little longer. Vacuum mercurial, from air. The effects gradually rose up in the same manner as before—but the passing spark at the Neg. *b* wire was not in the angle but at a spot midway between that and the end. Gradually the whole wire became feebly luminous—but that spot always gave a spark. The tongue (272) was there, very good and obedient to the magnet. The colour of the general light was white. The gradual education of the tube was evident. At last the stratification was distinct—i.e. there were maxima and minnima, the globes or clouds of light being large.



274. When the fingers or a conducting coating were brought to the glass of either of these tubes, especially at first when the light was uniform, there was remarkable repulsion of the light from the side touched to the opposite side. It seems to be a repulsion of the real discharge altogether, for the action of the magnet in sending the luminous column right or left *shews* that the discharge is in it rather than in the dark part. Must however test this point and compare it with the law of magnetic deflexion.

275. New No. 12. This tube has been rearranged by Casella, having been filled with oxygen and made vacuum by Air pump. Is now used for the first time except that the Neg. wire *b* has been educated before ( ). The discharge was through at once, but the Pos. light uniform up to the obscure space short of the Neg. wire. This Neg. wire was violet all over—the pos. wire was covered with a fine violet light. There were no strata in this oxygen tube.

276. No. 35. Is a new long tube, inch wide—filled with *oxygen* and made vacuum by Mercury—wires and all are new—not yet tried. The discharge through was easy and luminous from the first. Strata good, etc.—there was no particularity in it as an oxygen tube from other tubes, by magnet or otherwise—all was favourable. The fluorescent light at the neg. end appeared from the first and was very fine after growing up a little. It was very mobile by the magnet—but still I feel as if the source of the rays was at the Neg. wire.

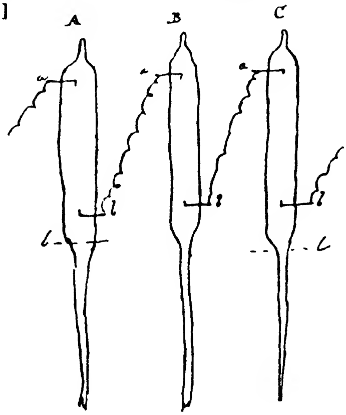


277. No. 23 (224) is a hydrogen long tube. Observed it for the fluorescence at the Neg. wire—that was very good as before and moved well by the Magnet—and I cannot make up my mind whether it is due to rays reflected to the place where it appears, or is the outline of an Electric plane crossing the medium. It is probably the reflection due to the concavity of the shoulder, but then it should disappear if the Neg. wire were in the middle part of the tube.

278*. A, B and C are three vacua from air, about 12 or 13 inches long in the wide part. Being put up in series and at common temperatures, A gave some stratification, B was to me doubtful, and C gave no appearance. Then C was plunged up to the line *l* in a mixture of pounded ice and muriatic acid—and after a short time it was full of very close strata from the Pos. wire to the dark Negative space and gave an excellent result of cooling. The tube A, being put into the bath up to *l* in like manner, was very much improved in the character of the strata, but they ran to and fro through and by each other.

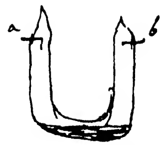
279. The tube B gave at common temperatures strata scarcely visible except with a lens, being very narrow especially near the Pos. wire. When put into the bath of acid and ice, the strata were very much improved. The series of light and dark layers on the

* [278]

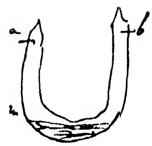


Negative wire was there and after them the dark Negative [space] – the whole very good.

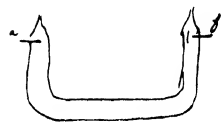
280. The effect of cold is manifest here, probably by condensing some vapour of moisture and so rarefying the medium.



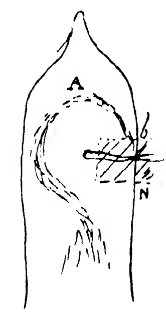
281. No. 18 (175, 260) is the short syphon Mercury tube. It gives very good striæ at common temperatures, and in this position these are continued from the pos. wire *a* over the mercury up to the negative dark space. Placed it thus in a bath of ice and salt at 0° F. When cold, the character of the striæ was improved; they were more separate and the space between was blacker. They were improved over the mercury as well as elsewhere. Continued the experiment for some time. When examined afterwards, there was a metallic deposit about the neg. wire, but this was mercury, probably dispersed from the wetted neg. wire – for when the body of mercury was brought up to it, it washed the film or dew off – which does not happen with the platinum deposit.



282. Then prepared a cold bath of *Ether and solid Carbonic acid* and placed the tube No. 18 in it in the position indicated. The Mercury within soon solidified and the thermometer fell to −85° F. Now the strata between the Pos. wire *a* and the first surface of the mercury at *m* much enlarged, being few by comparison and I think much less in light; but those over the solid mercury were numerous, bright and very much as at common temperatures. This difference in the part above *m* is remarkable.

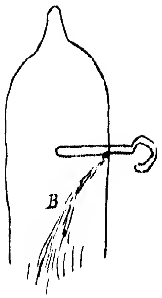


283. The carbonic acid bath was now applied to the smaller tube No. 31 (272) in the given position. Before being cooled there were no signs of stratification – the light was uniform and white. Nor when the cold was applied was there any sign of stratification, either with or without the Ruhmkorf condenser. The column of light was affected by the magnet. Applied a coating in place of the wire *b* so as to have the included current – it divided by the magnet as it ought to do. Ether [?] did not do for the coating as it insulated too well – used tin foil.



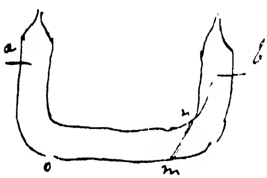
284. The Negative wire of the cold tube has a pale light over it, *not* the rosy reddish light of common temperatures, it being of course out of the bath and at common temperatures. There was a shoot of light from the foot of the Neg. *b* wire. When the horse shoe magnet was applied from the right, so that the poles were horizontal and on opposite sides of the tube, as marked by N – then

in one position of the poles the shoot rose up, made a curve round the end of the wire and descended into the space beneath, carrying on the electric discharge. But when the poles were reversed, the shoot proceeded from the same spot but went downwards at once. These directions accorded with the expected power of the magnet in deflecting that part of the current which set off from the spot in the platinum next the glass. There were no signs of stratification—and very little fluorescence (   ); the latter fact seems to shew that the fluorescence depends on the light on the wire—for now little or no light was there. The temperature was −97° F.



[A page in Gassiot's handwriting, included in the MS. here, records further experiments made by Gassiot with tubes 32 and 18, at the Royal Institution on March 19.]

285. Took the tube out of the bath placed it on flannel—continued the Ruhmkorf action and watched the changes as the temperature rose. There was soon a change—more light appeared. By applying the magnet, found that the shoot of light A identified itself with the former tongue of light (273), and that when B was produced by the right position of the magnet, the tongue appeared at the same time.



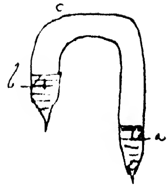
286. Also the rosy luminous covering appeared on the negative wire, and the fluorescence appeared at the same time. By the proper application of the magnet, the fluorescence could be thrown on to *m*, and I believe would have appeared further if the corner at *n* had not cut off the radiation. Looks wonderfully as if all depended upon the reflexion of the shoulder above *b* and the motion of the light on the wire *b* by the action of the magnet.

287. On examining the tube, it was found that a dew of mercury globules had gathered on the bottom from *o* to *m*. By the lamp it sublimed from place to place. The tube is not merely full of the vapour of mercury at common temperatures but there is a little excess of liquid mercury at that temperature.

288. Then placed this tube 31 in the same position in the air and applied a spirit lamp to the horizontal part of it. As the heat rose, the light became more brilliant and there were sparks or globules of light frequently appearing on the glass here and there, perhaps from globules of mercury. The fluorescence at the Negative wire was very fine—and travelled well by the magnet.

Sometimes puzzled to guess whether (or no) a plane of electric condition may exist there.

289. No. 18 is the short syphon Mercury tube (282). Hung it up in air thus—both the electrodes are mercury now. At common temperatures made *a* Positive—there was fine Pos. strata continued over to the Neg. obscurity and then the Negative glow upon the mercury at *b*. Applied the spirit lamp to the bend and the end *a*—this made the strata approach each other. As the quantity of mercury vapour increased, the light increased, and when the mercury at *a* was very hot and distilling and condensing all the way to *c*—the colour of the light was of a fine green and the dart of flame upwards from the surface of *a* very fine and luminous, but unstratified. This dart seemed bright white and the green more around it nearer to the glass. The cessation of stratification, beginning at the Pos. mercury, gradually extended to the end of the Pos. column—but the Negative obscurity remained.



290. Making *b* Positive and the hotter end *a* Negative, then a fine river of light ran through which continued until, by taking away the lamp, cooling was allowed; and then gradually close striæ appeared. When the tube was entirely cooled, it returned to its first state. The neg. terminal then resumed its rose coloured layer of light and the strata were about $\frac{1}{10}$ or $\frac{1}{12}$ of an inch apart in the Pos. column.

291. As to the Positive and Negative mercury surfaces. It appears that Pos. metal and Neg. insulating medium *exchange with facility*. But that Pos. medium and negative metal *exchange with much difficulty*. Exp.[1] 1600, 1480, 1525.

292. 22 *March*. Wrote to Gassiot proposing to combine the Voltaic battery current and the Ruhmkorf current through the vacuum tubes—connecting both continuously with the tube—or connecting V. battery with tube and making tapping contact with the Ruhmkorf—or else making continuous contact of the Ruhmkorf and connecting the V. Battery by tapping contacts.

[Here follow nine pages of MS. in Gassiot's handwriting, the pages (but not the paragraphs) numbered by Faraday in continuation of his own notes. The experiments recorded were made by Gassiot during the period March 22 to April 23, 1858, and on the latter date he worked at the Royal Institution.]

[1] Experimental Researches.

25 NOVR. 1861.

Steinheill's[1] apparatus up in my room and in order.

For sources of light used two platina terminations placed thus before the object end of the telescope, at distances from half an inch to two inches from the slit—the slit also being vertical.

Connected these wires with the Holland Ruhmkorff coil—and excited that by three plates of its own battery.

All acted very well and the different parts of the spectrum shewed at different heights, the rays from the upper or the middle or the lower part of the discharge inverted. These parts have different character through the ignition of the wire ends, etc. Reversion of the current was very convenient and useful in this examination.

The discharge between the platinum wires gave its own rays—well separated by the apparatus.

By moistening the ends of these wires with saturated solutions of Salts of Soda, Baryta, strontia, lithia, etc. the peculiar rays due to these substances came out well, but did not last long—rewetting the ends reproduced them.

Baryta by its green and blue rays—Strontia by its blue ray—and lithia by its fine red and other rays—will be useful in further experiments.

A Nichol's prism was placed between the platinum wires and the object end slit and another as analyser on the eyepiece of the observing telescope. They answered their purpose very well with a lithium light—polarizing and analysing the ray in due order. In one position of the analyser, lines, many, were observed crossing the line of colour light—but these moved round with the analyzer and not with the polarising prism—they were due I think to a set of reflections in the former.

I put up the Steatite burner with a strong yellow soda flame, then between it and the slit the platina wires with soda salt on them. There was no apparent object, but the yellow light of the wires was added to the yellow light of the gas burner.

[1] Presumably C. A. von Steinheil.

Steinhill's app. up–associated with E.M.: good–urged by 10 pr. plates–good action.

Also the Ruhmkorf–and the platina points–2 pr. plates. Discharge is mixed, being part bright and part haze.

When course of current in the Ruhmkorf is in one direction and discharge at *x* occurs between platina points–then when the interrupter is most open the sparks are bluest and most distinct as spark and least hazy. As the interrupter is screwed up, its interval lessens, the sparks become more powerful as discharges–more electy. passes–and there is much haze and much discharge of E. in the hazy parts around the sparks–the latter seem then even to carry less Electy.

There is a difference between the two ends of the discharge–red or yellow and violet concentration. If the Ruhmkorff current be reversed, these terminations are reversed (the spark being upright). Otherwise the general character of the spark discharge is the same whichever way the current passes.

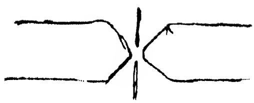
When the interrupter is screwed out and open, and the sparks are blue with little haze, then the spectrum in the Steinheill apparatus is pale and bluish the yellow and red rays being almost wanting; and that is the case whether the current is sent in one direction or the other, except that in one direction the red and yellow are almost entirely wanting and *a blue* is very fine, whereas in the other direction that blue and the general blue is not so fine and red and yellow faintly appears.

On screwing up the interruptor–more electricity passes and the hazy sparks appear. Then the spectrum in the Steinhiell exhibits yellow very bright and red, in addition to the blues, etc., and it does this whichever way the current is going through the Ruhmkorf, only with the difference before spoken of. The difference seems to depend upon the position of the spark discharge to the slit, for it is probable that each extremity is not equally placed in relation to the slit, and if so, changing the direction of the spark will make a difference.

With interruptor still more screwed up and the sparks still more hazy–the appearance in the Steinheill was more marked by the appearance of yellow and red rays. The same whichever way the current went, but with the same difference as before.

It seems as if the warm rays come from the luminous haze, which to the naked eye has a warm coloured light.

When the Rhumkorf discharge is first sent one way, the yellow and red appear with much brightness and power, but manifestly sinks as one counts 1, 2, 3, etc. and very shortly sinks in power; then reversing the Ruhmkorf current, the red and yellow shine out with much increased brilliancy, to sink in turn; reversing the current again, they appear brilliant and quickly sink, and so on continually. The continued discharge one way or the other lessens the ability to pour out the red and yellow rays. I think the same may be said of the blue and other rays but in a lesser degree. They are indeed altogether of lesser brightness.



Arranged the Ruhmkorf terminals so that the discharge was between the poles of the Electro magnet and adjusted the interruptor so that the discharge was all pale spark and with no haziness or flame about it. Now put on the Ruhmkorf and then made magnet--there was *no apparent* deviation of the course of the sparks they appeared to be undisturbed in direction. Then screw up the interruptor so as to give a little haze round the sparks; on putting on the E. Magnet, this haze was increased in some degree. It was thrown in between the poles or out from between them according as the Ruhmkorf current was in one direction or the other, and it was the old phenomenon of the deflection of the current by the magnetic poles. The haze, i.e. the flaming luminous part of the discharge, was collected into a flat plate of a circular or crescent form, thin and perpendicular to the magnetic axis–thrown in or out so as to be quite distinct from the place of the sparks, which remained undisturbed in their course and place.

When the interrupter was screwed up close so as to give a still more hazy spark and abundant current, then the haze and its deflection was still greater and finer, and even the bright sparks were shewn to be deflected a little in the same direction as the luminous arc. I think the magnet increased the bulk of the luminous cloud, but it might be only its disposition into a compact flat discharge–its collection from all sides to *one* side.

I do not find by a slip of paper that there is any draught either *in* or *out* in the direction that the arc seems to blow. It does not

seem to depend upon any motion of the air, but to be a place or line of discharge determined by the forces, not the transference of the charged and discharging air to a distance.

28 JANY. 1862.

Apparatus up as before, same E. Magnet but with 20 pair of plates. But the Ruhmkorf was dismissed and in place of it the Gas light used and the platinum wires with salts on it introduced. The poles were as before—pointed ends about $\frac{3}{4}$ of an inch apart and the flame between them.

When the flame was between the poles—the making of the Magnet did not change its form or direction—and there was no appearance of change in the lines when magnet made or broken.

Used Chlo. sodium on the platinum wire—still there was no appearance of change in the luminous lines in any way by the magnetism.

Poles $\frac{1}{3}$ of inch apart. No magnetic effect either with or without the Sodium.

Used Chloride *barium*—no effect.

Chloride Strontium—no effect.

Lithia—nothing.

12 MARCH 1862.

Apparatus as on last day (28 Jany.) but only 10 pr. of Voltaic battery for the Electromagnet.

The colourless Gas flame ascended between the poles of the Magnet and the salts of Sodium, Lithium, etc. were used to give colour. A Nicol's polarizer was placed just before the intense magnetic field and an analizer at the other extreme of the apparatus. Then the E. Magnet was made and unmade, but not the slightest trace of effect on or change of the lines in the spectrum were observed in any position of the Polarizer or analyzer.

Two other pierced poles were adjusted at the magnet—the coloured flame established between them, and only that ray taken up by the optic apparatus which came to it along the axis of the poles, i.e. in the magnetic axis or line of magnetic force. Then the Electro magnet was excited and rendered neutral; but not the slightest effect on the polarized or unpolarized ray was observed.